Acerca de este libro

Esta es una copia digital de un libro que, durante generaciones, se ha conservado en las estanterías de una biblioteca, hasta que Google ha decidido escanearlo como parte de un proyecto que pretende que sea posible descubrir en línea libros de todo el mundo.

Ha sobrevivido tantos años como para que los derechos de autor hayan expirado y el libro pase a ser de dominio público. El que un libro sea de dominio público significa que nunca ha estado protegido por derechos de autor, o bien que el período legal de estos derechos ya ha expirado. Es posible que una misma obra sea de dominio público en unos países y, sin embargo, no lo sea en otros. Los libros de dominio público son nuestras puertas hacia el pasado, suponen un patrimonio histórico, cultural y de conocimientos que, a menudo, resulta difícil de descubrir.

Todas las anotaciones, marcas y otras señales en los márgenes que estén presentes en el volumen original aparecerán también en este archivo como testimonio del largo viaje que el libro ha recorrido desde el editor hasta la biblioteca y, finalmente, hasta usted.

Normas de uso

Google se enorgullece de poder colaborar con distintas bibliotecas para digitalizar los materiales de dominio público a fin de hacerlos accesibles a todo el mundo. Los libros de dominio público son patrimonio de todos, nosotros somos sus humildes guardianes. No obstante, se trata de un trabajo caro. Por este motivo, y para poder ofrecer este recurso, hemos tomado medidas para evitar que se produzca un abuso por parte de terceros con fines comerciales, y hemos incluido restricciones técnicas sobre las solicitudes automatizadas.

Asimismo, le pedimos que:

+ *Haga un uso exclusivamente no comercial de estos archivos* Hemos diseñado la Búsqueda de libros de Google para el uso de particulares; como tal, le pedimos que utilice estos archivos con fines personales, y no comerciales.

+ *No envíe solicitudes automatizadas* Por favor, no envíe solicitudes automatizadas de ningún tipo al sistema de Google. Si está llevando a cabo una investigación sobre traducción automática, reconocimiento óptico de caracteres u otros campos para los que resulte útil disfrutar de acceso a una gran cantidad de texto, por favor, envíenos un mensaje. Fomentamos el uso de materiales de dominio público con estos propósitos y seguro que podremos ayudarle.

+ *Conserve la atribución* La filigrana de Google que verá en todos los archivos es fundamental para informar a los usuarios sobre este proyecto y ayudarles a encontrar materiales adicionales en la Búsqueda de libros de Google. Por favor, no la elimine.

+ *Manténgase siempre dentro de la legalidad* Sea cual sea el uso que haga de estos materiales, recuerde que es responsable de asegurarse de que todo lo que hace es legal. No dé por sentado que, por el hecho de que una obra se considere de dominio público para los usuarios de los Estados Unidos, lo será también para los usuarios de otros países. La legislación sobre derechos de autor varía de un país a otro, y no podemos facilitar información sobre si está permitido un uso específico de algún libro. Por favor, no suponga que la aparición de un libro en nuestro programa significa que se puede utilizar de igual manera en todo el mundo. La responsabilidad ante la infracción de los derechos de autor puede ser muy grave.

Acerca de la Búsqueda de libros de Google

El objetivo de Google consiste en organizar información procedente de todo el mundo y hacerla accesible y útil de forma universal. El programa de Búsqueda de libros de Google ayuda a los lectores a descubrir los libros de todo el mundo a la vez que ayuda a autores y editores a llegar a nuevas audiencias. Podrá realizar búsquedas en el texto completo de este libro en la web, en la página `http://books.google.com`

This is a reproduction of a library book that was digitized by Google as part of an ongoing effort to preserve the information in books and make it universally accessible.

Google™ books

https://books.google.com

LSoc1801.30

Harvard College Library

FROM THE FUND OF

CHARLES MINOT

(Class of 1828).

Received 5 July, 1892.

NOTICES
OF THE
PROCEEDINGS
AT THE
MEETINGS OF THE MEMBERS
OF THE
Royal Institution of Great Britain,
WITH
ABSTRACTS OF THE DISCOURSES
DELIVERED AT
THE EVENING MEETINGS.

VOLUME XI.
1884—1886.

C,

LONDON:
PRINTED BY WILLIAM CLOWES AND SONS, LIMITED,
STAMFORD STREET AND CHARING CROSS.
1887.

~~FX 302~~
LSoc 1801.30
JUL 5 1892
Minot fund.

Patron.

HER MOST GRACIOUS MAJESTY

QUEEN VICTORIA.

Vice-Patron and Honorary Member.

HIS ROYAL HIGHNESS

THE PRINCE OF WALES, K.G. F.R.S.

President—THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. LL.D.
Treasurer—HENRY POLLOCK, Esq.—*V.P.*
Honorary Secretary—SIR FREDERICK BRAMWELL, D.C.L. F.R.S.—*V.P.*

MANAGERS. 1886–87.

Sir Frederick Abel, C.B. D.C.L. F.R.S.—*V.P.*
Sir William Bowman, Bart, LL.D. F.R.S.—*V.P.*
Joseph Brown, Esq. Q.C.
Sir James Crichton Browne, M.D. LL.D. F.R.S.
William Crookes, Esq. F.R.S.
Henry Doulton, Esq.
Sir William Withey Gull, Bart, M.D. D.C.L. F.R.S.
Right Hon. The Lord Halsbury.—*V.P.*
William Huggins, Esq. D.C.L. LL.D. F.R.S.—*V.P.*
Alfred Bray Kempe, Esq. M.A. F.R.S.
Sir John Lubbock, Bart. M.P. D.C.L. LL.D. F.R.S.—*V.P.*
Hugo W. Müller, Esq. Ph.D. F.R.S.
Sir Frederick Pollock, Bart. M.A.—*V.P.*
John Rae, M.D. LL.D. F.R.S.
Lord Arthur Russell.

VISITORS. 1886–87.

Shelford Bidwell, Esq. M.A. F.R.S.
Stephen Busk, Esq.
Michael Carteighe, Esq. F.C.S.
Arthur Herbert Church, Esq. M.A. F.C.S.
Vicat Cole, Esq. R.A.
William Henry Domville, Esq.
James Edmunds, M.D. F.C.S.
Charles Hawksley, Esq. M.I.C.E.
Alfred Gutteres Henriques, Esq. F.G.S.
David Edward Hughes, Esq. F.R.S.
George Matthey, Esq. F.R.S.
John W. Miers, Esq.
Lachlan Mackintosh Rate, Esq. M.A.
William Chandler Roberts-Austen, Esq. F.R.S.
Alexander Siemens, Esq.

Professors.

Professor of Natural Philosophy—JOHN TYNDALL, Esq. D.C.L. LL.D. F.R.S. &c.
Fullerian Professor of Chemistry—JAMES DEWAR, Esq. M.A. F.R.S. Jacksonian Professor of Natural Experimental Philosophy, Univ. Cambridge.
Fullerian Professor of Physiology—ARTHUR GAMGEE, M.D. F.R.S.

Keeper of the Library and Assistant Secretary—Mr. Benjamin Vincent.
Assistant in the Library—Mr. Henry Young.
Clerk of Accounts and Collector—Mr. Henry C. Hughes.
Assistant in the Chemical Laboratory—Mr. R. N. Lennox.

CONTENTS.

1884.

Page

Feb. 8.—**George J. Romanes**, Esq.—The Darwinian Theory of Instinct 131

„ 29.—**Professor D. E. Hughes**.—Theory of Magnetism 1

March 3.—General Monthly Meeting 11

„ 7.—**C. Vernon Boys**, Esq.—Bicycles and Tricycles in Theory and Practice 13

„ 14.—**J. N. Langley**, Esq.—The Physiological Aspect of Mesmerism 25

„ 21.—**Matthew Arnold**, Esq.—Emerson (*no Abstract*) .. 43

„ 28.—**Professor Osborne Reynolds**.—The Two Manners of Motion of Water 44

April 4.—**Professor T. G. Bonney**.—The Building of the Alps 53

„ 7.—General Monthly Meeting 68

„ 25.—**Walter Besant**, Esq.—The Art of Fiction .. 70

May 1.—Annual Meeting 84

„ 2.—**Professor J. W. Judd**.—Krakatoa 85

„ 5.—General Monthly Meeting 88

„ 9.—**Professor W. Robertson Smith**.—Mohammedan Mahdis 147

1884. Page

May 16.—PROFESSOR W. ODLING.—The Dissolved Oxygen of Water (*no Abstract*) 90

„ 23.—DAVID GILL, Esq.—Recent Researches on the Distances of the Fixed Stars, and Some Future Problems in Sidereal Astronomy 91

„ 30.—MONSIEUR E. MASCART.—Sur Les Couleurs. (In French.) 107

June 2.—General Monthly Meeting 116

„ 6.—WILLOUGHBY SMITH, Esq.—Volta-Electric and Magneto-Electric Induction 119

„ 13.—(Extra Evening.)—PROFESSOR DEWAR.—Researches on Liquefied Gases 148

July 7.—General Monthly Meeting 153

Nov. 3.—General Monthly Meeting 155

Dec. 1.—General Monthly Meeting 159

1885.

Jan. 16.—PROFESSOR TYNDALL.—On Living Contagia .. 161

„ 23.—PROFESSOR H. N. MOSELEY.—The Fauna of the Sea-shore 168

„ 30.—PROFESSOR ERNST PAUER.—A short review of the works of Living Composers for the Pianoforte .. 171

Feb. 2.—General Monthly Meeting 175

„ 6.—G. JOHNSTONE STONEY, Esq.—How Thought presents itself in Nature 178

„ 13.—SIR JOHN LUBBOCK, Bart. M.P.—The Forms of Leaves 197

„ 20.—WILLIAM HUGGINS, Esq.—The Solar Corona .. 202

„ 27.—PROFESSOR E. RAY LANKESTER.—A Marine Biological Laboratory 215

1885. Page
March 2.—General Monthly Meeting 216
„ 6.—Charles T. Newton, C.B.—The German Discoveries at Pergamus (*no Abstract*) 217
„ 13.—Sir Frederick Abel, C.B.—Accidental Explosions produced by non-explosive Liquids 218
„ 20.—Professor A. W. Rücker.—Liquid Films 243
„ 27.—Victor Horsley, Esq.—The Motor Centres of the Brain, and the Mechanism of the Will 250
April 6.—General Monthly Meeting 263
„ 17.—Professor S. P. Langley.—Sunlight and the Earth's Atmosphere. 265
„ 24.—William Carruthers, Esq.—British Fossil Cycads and their relation to Living Forms (*no Abstract*) 283
May 1.—Annual Meeting 283
„ 1.—The Right Hon. Lord Rayleigh.—Water Jets and Water Drops (*no Abstract*) 284
„ 4.—General Monthly Meeting 285
„ 8.—W. F. R. Weldon, Esq.—On Adaptation to Surroundings as a Factor in Animal Development (*no Abstract*) 287
„ 15.—Professor Burdon Sanderson.—Cholera: its Cause and Prevention 288
„ 22.—Walter H. Pollock, Esq.—Garrick as an Actor .. 304
„ 29.—J. J. Coleman, Esq. and Professor J. G. McKendrick.—The Mechanical Production of Cold, and the Effects of Cold on Microphytes .. 305
June 1.—General Monthly Meeting 317
„ 5.—Professor Dewar.—Liquid Air and the Zero of absolute Temperature (*no Abstract*) 318
July 6.—General Monthly Meeting 319
Nov. 2.—General Monthly Meeting 321

1885. Page
Dec. 7.—General Monthly Meeting 325
„ 29.—Notes on PROFESSOR DEWAR'S Lectures on the
et seq. Story of a Meteorite 328

1886.

Jan. 22.—PROFESSOR TYNDALL.—Thomas Young 553
„ 29.—SIR WILLIAM THOMSON.—Capillary Attraction .. 483
Feb. 5.—T. PRIDGIN TEALE, Esq.—The Principles of Domestic Fire-place Construction 338
„ 12.—PROFESSOR OSBORNE REYNOLDS.—Experiments showing Dilatancy, a property of Granular Material, possibly connected with Gravitation 354
„ 19.—PROFESSOR W. H. FLOWER.—The Wings of Birds .. 364
„ 26.—A. A. COMMON, Esq.—Photography as an Aid to Astronomy 367
March 1.—General Monthly Meeting 376
„ 5.—PROFESSOR ALEXANDER MACALISTER.—Anatomical and Medical Knowledge of Ancient Egypt .. 378
„ 12.—REGINALD STUART POOLE, Esq.—The Discovery of the Biblical Cities of Egypt 384
„ 19.—W. H. M. CHRISTIE, Esq.—Universal Time .. 387
„ 26.—W. CHANDLER ROBERTS-AUSTEN, Esq.—On Certain Properties common to Fluids and Solid Metals .. 395
April 2.—HOWARD GRUBB, Esq.—Telescopic Objectives and Mirrors: their Preparation and Testing .. 413
„ 5.—General Monthly Meeting 433
„ 9.—WILLIAM ANDERSON, Esq.—New Applications of the Mechanical Properties of Cork to the Arts .. 436
„ 16.—PROFESSOR SIR HENRY E. ROSCOE, M.P.—Recent Progress in the Coal Tar Industries 450

1886. Page
May 1.—Annual Meeting 467
„ 3.—General Monthly Meeting 468
„ 7.—FREDERICK SIEMENS, Esq.—Dissociation Temperatures 471
„ 14.—PROFESSOR JOHN MILLAR THOMSON.—Suspended Crystallisation 508
„ 21.—SIR JOHN LUBBOCK, BART. M.P.—The Forms of Seedlings: the causes to which they are due .. 517
„ 28.—PROFESSOR OLIVER LODGE.—Electrical Deposition of Dust and Smoke 520
June 4.—WALTER H. GASKELL, M.D.—The Sympathetic Nervous System 530
„ 7.—General Monthly Meeting 538
„ 11.—PROFESSOR DEWAR.—Recent Researches on Meteorites 541
July 5.—General Monthly Meeting 589
Nov. 1.—General Monthly Meeting 591
Dec. 6.—General Monthly Meeting 594
Index to Volume XI. 597

PLATES.

Page
Distribution of Solar Energy 279
On Dissociation Temperatures 475, 481
Apparatus for Solidifying Oxygen 550

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, February 29, 1884.

Sir William Bowman, Bart. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Professor D. E. Hughes, F.R.S. *M.R.I.*

Theory of Magnetism.

The theory of magnetism, which I propose demonstrating this evening, may be termed the mechanical theory of magnetism, and, like the now well-established mechanical theory of heat, replaces the assumed magnetic fluids and elementary electric currents by a simple, symmetrical, mechanical motion of the molecules of matter and ether.

That magnetism is of a molecular nature has long been accepted, for it is evident that, no matter how much we divide a magnet, we still have its two poles in each separate portion, consequently we can easily imagine this division carried so far that we should at last arrive at the molecule itself possessing its two distinctive poles, consequently all theories of magnetism attempt some explanation of the cause of this molecular polarity, and the reason for apparent neutrality in a mass of iron.

Coulomb and Poisson assume that each molecule is a sphere containing two distinct magnetic fluids, which in the state of neutrality are mixed together, but when polarised are separated from each other at opposite sides; and, in order to explain why these fluids are kept apart as in a permanent magnet, they had to assume, again, that each molecule contained a peculiar coercive force, whose functions were to prevent any change or mixing of these fluids when separated.

There is not one experimental evidence to prove the truth of this assumption; and as regards coercive force, we have direct experimental proof opposing this view, as we know that molecular rigidity or hardness, as in tempered steel, and molecular freedom or softness, as in soft iron, fulfil all the conditions of this assumed coercive force.

Ampère's theory, based upon the analogy of electric currents, supposes elementary currents flowing around each molecule, and that in the neutral state these molecules are arranged hap-hazard in all directions, but that magnetisation consists in arranging them symmetrically.

The objections to Ampère's theory are numerous. 1st. We have no knowledge or experimental proof of any elementary electric currents continually flowing without any expenditure of energy. 2nd. If we admit the assumption of electric currents around each molecule, the molecule itself would then be electro-magnetic, and the question still remains, What is polarity? Have the supposed electric currents separated the two assumed magnetic fluids contained in the molecule, as in Poisson's theory? or are the electric currents themselves magnetic, independent of the iron molecule?

In order to produce the supposed heterogeneous arrangement of neutrality, Ampère's currents would have either to change their position upon the molecule, and have no fixed axis of rotation, or else the molecule, with its currents and polarities, would rotate, and thus be acting in accordance with the theory of De la Rive. 3rd. This theory does not explain why (as in the case of soft iron) polarity should disappear whenever the exciting cause is removed, as in the case of transient magnetisation. It would thus require a coercive force in iron to cause exactly one-half of the molecules to instantly reverse their direction, in order to pass from apparent external polarity to that of neutrality.

The influence of mechanical vibrations and stress upon iron in facilitating or discharging its magnetism, as proved by Matteucci, 1847, in addition to the discovery by Page, 1837, of a molecular movement taking place in iron during its magnetisation, producing audible sounds, and the discovery by Dr. Joule, 1842, of the elongation of iron when magnetised, followed by the discoveries of Guillemin, that an iron bar bent by a weight at its extremity would become straight when magnetised; also that magnetism would tend to take off twists or mechanical strains of all kinds—together with the researches of Matteucci, Marianini, De la Rive, Sir W. Grove, Faraday, Weber, Wiedemann, Du Moncel, and a host of experimenters, including numerous published researches by myself—all tend to show that a mechanical action takes place whenever a bar of iron is magnetised, and that the combined researches demonstrate that the movement is that of molecular rotation.

De la Rive was the first to perceive this, and his theory, like those of Weber, Wiedemann, Maxwell, and others, is based upon molecular rotation. Their theories, however, were made upon insufficient data, and have proved to be wrong as to the assumed state of neutrality, and right only where the experimental data clearly demonstrated rotation.

I believe that a true theory of magnetism should admit of complete demonstration, that it should present no anomalies, and that all the known effects should at once be explained by it.

From numerous researches I have gradually formed a theory of magnetism entirely based upon experimental results, and these have led me to the following conclusions:—

1. That each molecule of a piece of iron, as well as the atoms of all matter, solid, liquid, gaseous, and the ether itself, is a separate and independent magnet, having its two poles and distribution of magnetic polarity exactly the same as its total evident magnetism when noticed upon a steel bar-magnet.

2. That each molecule can be rotated in either direction upon its axis by torsion, stress, or by physical forces such as magnetism and electricity.

3. That the inherent polarity or magnetism of each molecule is a constant quantity like gravity; that it can neither be augmented nor destroyed.

4. That when we have external neutrality, or no apparent magnetism, the molecules arrange themselves so as to satisfy their mutual attraction by the shortest path, and thus form a complete closed circuit of attraction.

5. That when magnetism becomes evident, the molecules and their polarities have all rotated symmetrically, producing a north pole if rotated in a given direction, or a south pole if rotated in the opposite direction. Also, that in evident magnetism we have still a symmetrical arrangement, but one whose circles of attraction are not completed except through an external armature joining both poles.

6. That we have permanent magnetism when the molecular rigidity, as in tempered steel, retains them in a given direction, and transient magnetism whenever the molecules rotate in comparative freedom, as in soft iron.

Experimental Evidences.

In the above theory the coercive force of Poisson is replaced by molecular rigidity and freedom; and as the effect of mechanical vibrations, torsion, and stress upon the apparent destruction and facilitation of magnetism is well known, I will, before demonstrating the more serious parts of the theory, make a few experiments to prove that molecular rigidity fulfils all the requirements of an assumed coercive force.

I will now show you that if I magnetise a soft iron rod, the slightest mechanical vibration reduces it to zero; whilst in tempered steel or hard iron, the molecules are comparatively rigid, and are but slightly affected. The numerous experimental evidences which I shall show prove that whilst the molecules are not completely rigid in steel, they are comparatively rigid when compared with the extraordinary molecular freedom shown in soft iron. (*Experiments shown.*)

If I now take a bottle of iron filings, I am enabled to show how completely rigid they appear if not shaken; but the slightest motion allows these filings to rotate and short circuit themselves, thus producing apparent neutrality. Now I will restore the lost magnetism by letting the filings slowly fall on each other under the influence of the earth's magnetic force; and here we have an evident proof of rotation producing the result, as we can ourselves perceive the arrangement of the filings. (*Experiment shown.*)

If I take this extremely soft bar of iron, you notice that the slightest mechanical tremor allows molecular rotation, and consequent loss or change of polarity; but if I put a slight strain on this bar, so as to fasten each molecule, they cannot turn with the same freedom as before, and they now retain their symmetrical polarity like tempered steel, even when violently hammered. (*Experiment shown.*)

We can only arrive at one conclusion from this experiment, viz. that the retention of apparent magnetism is simply due to a frictional resistance to rotation; and whenever this frictional resistance is reduced, as when we take off a mechanical strain, or by making the bar red hot, the molecules then rotate with an almost inconceivable freedom from frictional resistance.

Conduction.

You notice that if I place this small magnet at several inches' distance from the needle, it turns in accordance with the pole presented. How is the influence transmitted from the magnet to the needle? It is through the atmosphere and the ether, which is the intervening medium. I have made a long series of researches on the subject, involving new experimental methods, the results of which are not yet published. One result, however, I may mention. We know that iron cannot be magnetised beyond a certain maximum, which we call its saturation point. It has a well-defined curve of rise to saturation, agreeing completely with a curve of force produced by the rotation of a bar magnet, the force of which was observed from a fixed point. I have completely demonstrated by means of my magnetic balance (*shown in the Library*) that our atmosphere, as well as Crooke's vacuum, has its saturating point exactly similar in every respect to that of iron: it has the same form through every degree. We cannot reduce nor augment the saturating point of ether; it is invariable, and equals the finest iron. We may, however, easily reduce that of iron by introducing frictional resistance to the free motion of its molecules.

From consideration of the ether having its saturating point, I am forced to the conclusion that it could only be explained by a similar rotation of its atoms as demonstrable in iron.

Reflection would teach us that there cannot be two laws of magnetism, such as one of vibrations in the ether and rotations in iron. We cannot have two correct theories of heat, light, or magnetism; the mode of motion in the case of magnetism being rotation, and not vibration.

Let us observe this saturation point of the atmosphere compared with iron. I pass a strong current of electricity in this coil. The coil is quite hot, so we are very near its saturation. I now place this coil at a certain distance from the needle (8 inches); we have now a deflection of 45° on the needle. I now introduce this iron core, exactly fitting the interior previously filled by the ether and atmosphere. Its force is much greater, so I gradually remove this coil to a distance, where I find the same deflection as before (45°). This happens to be at twice the distance, or 16 inches, so we know, according to the law of inverse squares, that the iron has four times the magnetic power of the atmosphere. But this is only true for this piece of iron: with extremely fine specimens of iron I have been enabled to increase the force of the coil forty times, whilst with manganese steel containing 10 per cent. of manganese it was only 30 per cent. superior. We see here that the atmosphere is extremely magnetic. Let us replace the solid bar by iron filings. We now only have twice the force. Replace this by a bottle of sulphate of iron in a liquid state: it is now a mere fraction superior to the atmosphere; and if we were still further to separate the iron molecules, as in a gaseous state, it is reasonable to suppose that if we could isolate the iron gas from that of ether, that iron gas would be strongly diamagnetic, or have far less magnetic capacity than ether, owing to the great separation of its molecules. These are assumptions, but they are based upon experimental evidences, which give it value.

Let us quit the domain of assumption to enter that of demonstration. Here I have a long bar of neutral iron. If I place this small magnet at one end, we notice that its pole has moved forward three inches, having a consequent point at that place. Let us now vibrate this rod, and you notice the slow but gradual creeping of the conduction until at the end of two seconds it has reached 14 inches. The molecules have been freed from frictional resistance by the mechanical vibrations, and have at once rotated all along the bar. (*Experiment shown.*) Let us repeat this experiment by heating the rod to red heat. You notice the gradual creeping or increased conduction as the heat allows greater molecular freedom. (*Experiment shown.*) Let us now again repeat this experiment by sending a current of electricity through the bar. You notice the instant that I touch the bar with this wire, conveying the current through it, that we have identically the same creeping forwards, no matter what direction of the current. (*Experiment shown.*) If you simply looked at the effects produced, you could not tell which method I had employed; either mechanical vibrations, heat vibrations, or electrical currents. Consequently, knowing the two first to be modes of motion, it is fair to assume that an electrical current is a mode of motion, the manner of which is at present unknown; but that there is a molecular disturbance in each case is evident from the experiments shown.

Neutrality.

If I take this bar of soft iron, introduce it in the coil, and pass a strong electric current though the coil, you notice that it is intensely magnetic, holding up this large armature of iron and strongly deflecting the observing needle. I now interrupt the current, the armature falls, and the needle only shows traces of the previous intense magnetisation. What has become of this polarity? or what has caused this sudden neutrality? Coulomb supposes that the magnetic fluids have become mixed in each molecule, thus neutralising each other. Ampère supposes that the elementary currents surrounding each molecule have become heterogeneous. De la Rive, Wiedemann, Weber, Maxwell, and all up to the present time have accounted for this disappearance as a case of mixture of polarities or heterogeneous arrangement.

My researches proved to me that neutrality was a symmetrical arrangement; I stated this in my paper upon the theory of magnetism to the Royal Society last year. I have since made a long series of researches upon this question, and my paper upon this subject will shortly be read at the Royal Society. This paper will demonstrate beyond question—1. That a bar of iron under the influence of a current or other magnetising force is more strongly polarised on the outside than in the interior; that its degree of penetration follows the well-defined law of inverse squares, up to the saturation point of each successive layer. 2. The instant that the current ceases, a reaction takes place, the stronger outside reacting upon the weaker inside, completely reversing it, until its reversed polarity exactly balances the external layers.

We might here suppose that there existed two distinct polarities at the same end of a neutral bar, but this is only partially true, as the rotation of the molecules from the inside to the exterior is a gradual, well-defined curve, perfectly marked, as shown in the diagrams. (*Diagrams explained.*) We see from these that in a large solid bar the reversed polarity would be in the interior, but in a thin bar under an intense field, the reversed polarity would be on the outside. Thus a bar which had previously strong north polarity under an external influence would, the instant it formed its neutrality, have a north polarity in the interior covered or rendered neutral by an equal south exterior, the sum of both giving the apparent neutrality that we notice. I must refer all interested upon this question to my paper shortly to be read, but I will make a few experiments to demonstrate this important fact.

If I take this piece of soft steel and magnetise it strongly, it has a strong remaining magnetism, or only partial neutrality. If I now heat this steel to redness, or put it into a state of mechanical vibration, the remaining magnetism almost entirely disappears, and we have apparent neutrality. This piece of steel being thin (½ millimetre), I know that the outside is reversed to its previous state. I place this piece of steel in a glass vase near the observing needle, and at present there seems no polarity. I now pour dilute nitric acid upon it, filling up the vase. The exterior is now being dissolved, and in a few minutes you will see a strong polarity in the steel, as the exterior reversed polarity is dissolved in the acid. (*Experiment shown.*)

Let us observe this by a different method. I take two strips of hard iron, and magnetise them both in the same direction.

If I place them together and then separate them, there seems no change, although in reality the mere contact produced a commencement of reversal. Let us vibrate them whilst together, allowing the molecules greater freedom to act as they feel inclined ; and now on separating we see that one strip has exactly the opposite polarity to the other, both extremely strong, but the sum of which, when placed together, is zero, or neutrality. (*Experiment shown.*)

Let us take two extremely soft strips placed together, and magnetised whilst together. On withdrawal of the inducing force, the rods are quite neutral. (*Experiment shown.*)

We now separate these strips, and find that one is violently polarised in one direction, whilst the other is equally strong in the reversed ; the sum of both being again zero.

We might suppose that the reaction is due to having separate bars. I will now demonstrate that this is not the case by magnetising this large ¾-inch bar with a magnetising force just sufficient to render the rod completely neutral when held vertically or under the earth's magnetic influence. (*Experiment shown.*)

You notice that it is absolutely neutral, all parts as well as the ends showing not the slightest trace of polarisation. I reverse this bar, and you perceive that it is now intensely polarised. This is due to the fact that the earth's influence uncovers or reverses the outside molecules, and consequently they are now of the same polarity as its interior. Upon reversing this rod, the magnetism again disappears, and re-appears if turned as previously. We have thus a rod which appears intensely magnetic when one of its ends is lowermost, whilst if that same end is turned upwards all traces of magnetism disappear. These and several other demonstrations which I shall now show you (proving the enormous influence which thickness of a bar has in the production of neutrality or its retention of magnetism) are simple lecture demonstrations. For the complete proof of my discovery of neutral curves I must refer you to my forthcoming paper upon this subject. (*Experiments shown proving the great influence of a thickness of a bar upon its retentive and neutral powers.*)

Inertia.

I have remarked in my researches that the molecules have true inertia, that they resist being put in motion, and if put in motion will vanquish an opposing resistance by their simple momentum. To illustrate this, I take this large ¾-inch bar, magnetise it so that its south pole is at the lowest end. We know that the earth's influence is to make the lower end north. I now gently strike it with a wooden mallet, and the rod immediately falls to zero. I continue these blows, but the rod obstinately refuses to pass the neutral line to become north, the reason being in so doing it would have to change the whole internal reversed curve that I have discovered. It requires now extremely violent and repeated blows from the mallet to make it obey the earth's influence.

Let us repeat this experiment by starting the molecule rapidly in the first instance. The rod is now magnetised south as before. I give one single sharp tap; the molecules run rapidly round, pass through neutrality, breaking up its curve, and arrive at once to strong north polarity. (*Experiment shown.*)

A very extraordinary effect is shown if we produce this effect by electricity; it then almost appears as if electricity itself had inertia. I take this bar of hard iron and magnetise it to a fixed degree. On the passage of the current, you notice that the magnetism seems to be increased as the needle increases its arc, but this is caused by the deflection of the electric current in the bar. The current is now obliged to travel in spirals, as my researches have proved to me that electricity can only travel at right angles to the magnetic polar direction of a molecule, consequently in all permanent magnets the current must pass at right angles to the molecule, and its path will be that of a spiral. Let us replace this bar by one from a similar kind of iron well annealed. The molecules here are in a great state of freedom. We now magnetise this rod to the same degree as in the previous case; the electric current now, instead of being deflected, completely rotates the molecules, and the needle returns to zero, all traces of external magnetism having ceased. The electricity on entering this bar should have been forced to follow a tortuous circular route; its momentum was, however, too great for the molecules, and they elected to turn, allowing the electricity to pass in a straight line through the bar. Thus, in the first instant, magnetism was the master directing the course of the current; in the last, it became its servant, obeying by turning itself to allow a straight path to its electric master. (*Experiment shown.*)

Superposed Magnetism.

It is well known that we can superpose a weak contrary polarity upon an internal one of an opposite name. I have been enabled thus to superpose twenty successive stratas of opposite polarities upon a single rod, by simply diminishing the force at each reversal. I was anxious to prepare a steel wire so that in its ordinary state it would be neutral, but that in giving it a torsion to the right one polarity would appear, whilst a torsion to the left would produce the opposite polarity. This I have accomplished by taking ordinary soft steel drill wire and magnetising it strongly whilst under a torsion to the right, and more feebly with an opposite polarity when magnetised under torsion to the left.

The power of these wires, if properly prepared, is most remarkable, being able to reverse their polarity under torsion, as if they were completely saturated; and they preserve this power indefinitely if not touched by a magnet. It would be extremely difficult to explain the action of the rotative effects obtained in these wires under any other theory than that which I have advanced; and the absolute external neutrality that we obtain in them when the polarities are changing we know, from their structure, to be perfectly symmetrical.

I was anxious to show some mechanical movement produced by molecular rotation, consequently I have arranged two bells that are struck alternately by a polarised armature put in motion by the double polarised rod I have already described, but whose position, at three centimetres distant from the axis of the armature, remains invariably the same. The magnetic armature consists of a horizontal light steel bar suspended by its central axle; the bells are thin wine-glasses, giving a clear musical tone loud enough, by the force with which they are struck, to be clearly heard at some distance. The armature does not strike these alternately by a pendulous movement, as we may easily strike only one continuously, the friction and inertia of the armature causing its movements to be perfectly dead-beat when not driven by some external force, and it is kept in its zero position by a strong directive magnet placed beneath its axle.

The mechanical power obtained is extremely evident, and is sufficient to put the sluggish armature in rapid motion, striking the bells six times per second, and with a power sufficient to produce tones loud enough to be clearly heard in all parts of the hall of the Institution.

There is nothing remarkable in the bells themselves, as they evidently could be rung if the armature was surrounded by a coil, and worked by an electric current from a few cells. The marvel, however, is in the small steel superposed magnetic wire producing by slight elastic torsions from a single wire, 1 millimetre in diameter, sufficient force from mere molecular rotation to entirely replace the coil and electric current. (*Experiment shown by ringing the bells by the torsion of a small $\frac{1}{16}$-inch wire placed 4 inches distant from bell-hammer.*)

Correlation of Forces.

There is at present a tendency to trace all physical forces to one, or rather a variation of modes of motion. In my last experiment the energy of my arm was transformed in the wire to molecular motion, producing evident polarity; this, again, acted upon the ether, putting the needle-hammer into mechanical motion. This by its impact upon the glass bells transformed its motions into sonorous vibrations; but this does not mean that we can convert directly sonorous vibrations into magnetism, or *vice versâ.*

Let us take this soft iron rod; it seems quite neutral, although we know that the earth's magnetism is trying to rotate its molecules to north polarity at its lowest extremity. We now put it in mechanical vibration by striking it gently with a wooden mallet; the molecules at once rotate, and we have the expected strong north polarity. Let us repeat this experiment by employing heat, and here, again, at red heat an equally strong north polarity appears.

Again we repeat, and simply pass an electric current of no matter what direction; again the same north pole appears. Thus these forces must be very similar in nature, and may be fairly presumed to be vibrations, or modes of motion, having no directive tendency except a slight one, as in the case of electricity. For the same three forces render the rod perfectly neutral, even when previously magnetised, when placed in a longitudinally neutral field, as east and west.

Motion of the molecules gives rise to external magnetism to a rod previously neutral, or renders it neutral when previously magnetised; in other words, it simply allows the molecules to obey an external directing influence; the only motion, therefore, is during a change of state or polarity. If there is constant polarity, there is no consequent motion of the molecules: in fact, the less motion of any kind that it can receive, the more perfect its retention of its previous position; consequently, constant magnetism cannot be looked upon as a mode of motion, neither vibratory nor rotatory; it is an inherent quality of each molecule, similar in its action to its chemical affinity, cohesion, or its polar power of crystallisation. A molecule of all kinds of matter has numerous endowed qualities; they are inherent, and special in degree to the molecule itself. I regard the magnetic endowed qualities of all matter or ether to be inherent, and that they are rendered evident by rotation to a symmetrical arrangement in which their complete polar attractions are not satisfied.

Time will not allow me to show how completely this view explains all the phenomena of electro-magnetism, diamagnetism, earth currents—in fact, all the known effects of magnetism—up to the original cause of the direction of the molecules of the earth. To explain the first cause of the direction of the molecules of the earth would rest altogether upon assumption as the first cause of the earth's rotation, and of all things down to the inherent qualities of the molecule itself.

The mechanical theory of magnetism which I have advocated seems to me as fairly demonstrable as the mechanical theory of heat, and it gives me great pleasure to have been allowed to present you with my views on the theory of magnetism.

[D. E. H.]

GENERAL MONTHLY MEETING,

Monday, March 3, 1884.

SIR FREDERICK POLLOCK, Bart. M.A. Manager and Vice-President, in the Chair.

Major-General William Howell Beynon,
Frederick William Blunt, Esq.
John Griffin Bristow, Esq. M.A.
Mrs. R. Brudenell Carter,
Mrs. J. T. Clover,
Frank M. Gowan, Esq.
Wynnard Hooper, Esq.
John Charles Medd, Esq.
Robert Muir, Esq.
Bonamy Mansell Power, Esq.
Mrs. William Crookes,
P. Donovan, Esq. J.P.
Lewis H. Edmunds, Esq. B.A.
Miss J. L. Reynolds,
Shirley Harris Salt, Esq.
George Nelson Strawbridge, Esq. F.Z.S.

were elected Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Palæontologia Indica: Series X. Vol. II. Part 6. 4to. 1884.

The Trustees of the British Museum—Catalogue of Romances in MSS. By H. L. D. Ward. Vol. I. 8vo. 1883.

Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Transunti. Vol. VIII. Fasc. 2, 3. 1883–4.

Academy of Natural Sciences, Philadelphia—Proceedings, 1883, Part 2. 8vo. 1883.

Alpine Club—The Alpine Journal, Nos. 1 to 16 and Nos. 21 to 83. 8vo. 1863-84.

American Academy of Arts and Sciences—Proceedings, Vol. XVIII. 8vo. 1883.

American Philosophical Society—Proceedings, No. 113. 8vo. 1883.
Transactions, Vol. XVI. Part 1. 4to. 1883.

Astronomical Society, Royal—Monthly Notices, Vol. XLIV. No. 3. 8vo. 1884.

Bankers, Institute of—Journal, Vol. V. Parts 1, 2. 8vo. 1884.

Boston Society of Natural History—Memoirs, Vol. III. Nos. 6, 7. 4to. 1883.
Proceedings, Vol. XXI. Part 4; Vol. XXII. Part 1. 8vo. 1883.

British Architects, Royal Institute of—Proceedings, 1883–4, Nos. 8, 9. 4to.

Canada Geological and Natural History Survey—Report of Progress for 1880-2, with Maps. 8vo. 1883.
Catalogue of Canadian Plants. Part 1, Polypetalæ. By J. Macoun. 8vo. 1883.

Chief Signal Officer, U.S. Army—Professional Papers of the Signal Service, Nos. 8 to 12. 4to. 1882.

Chemical Society—Journal for Feb. 1884. 8vo.
Index, Vols. XLIII. and XLIV. 8vo. 1883.

Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. IV. Part 1. 8vo. 1884.

Editors—American Journal of Science for Feb. 1884. 8vo.
Analyst for Feb. 1884. 8vo.
Athenæum for Feb. 1884. 4to.
Chemical News for Feb. 1884. 4to.
Engineer for Feb. 1884. fol.
Horological Journal for Feb. 1884. 8vo.
Iron for Feb. 1884. 4to.
Nature for Feb. 1884. 4to.
Revue Scientifique and Revue Politique et Littéraire for Feb. 1884. 4to.
Science Monthly, Illustrated, for Feb. 1884.
Telegraphic Journal for Feb. 1884. 8vo.

Franklin Institute—Journal, No. 698. 8vo. 1884.

Geographical Society, Royal—Proceedings, New Series, Vol. VI. No. 2. 8vo. 1884.

Geological Society—Quarterly Journal, No. 157. 8vo. 1884.

Glasgow University Observatory—Catalogue of 6415 Stars for 1870. By R. Grant. 4to. 1883.

Johns Hopkins University—American Chemical Journal, Vol. V. No. 6. 8vo. 1883.

Liverpool Literary and Philosophical Society—Proceedings, Vols. XXXV. XXXVI. and XXXVII. 8vo. 1880–3.

Medical and Chirurgical Society—Proceedings, New Series, No. 4. 8vo. 1883.

North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXIII. Part 2. 8vo. 1884.

Pharmaceutical Society of Great Britain—Journal, Feb. 1884. 8vo.

Photographic Society—Journal, New Series, Vol. VIII. No. 4. 8vo. 1884.

Plateau, Madame—Expériences sur les Lames Liquides Minces. 2e Note. Par J. Plateau. (Bul. Acad. Roy. de Belgique, t. VI.) 1883.
Sur l'Observation des Mouvements très Rapides. Par J. Plateau. (Bul. Acad. Roy. de Belgique. t. VI.) 1883.
Bibliographie Analytique des Principaux Phenomènes Subjectifs de la Vision. Par J. Plateau. (Mem. de l'Acad. Roy. de Belgique, t. XLV.) 1883.

Pogson, Miss E. Isis (the Reporter)—Report of the Meteorological Reporter to the Government of Madras, 1881–3. 8vo. 1882–3.

Preussische Akademie der Wissenschaften—Sitzungsberichte, XXXVIII.–LIII. 4to. 1883.

Secretary of State for the Colonies—Report on the Agricultural Resources of the Island of St. Helena. By D. Morris. 8vo. 1884.

Society of Arts—Journal, Feb. 1884. 8vo.

Telegraph Engineers, Society of—Journal, Vol. XII. No. 50. 8vo. 1884.

The Editor—Electrician's Directory, 1884. 8vo.

Tobin, W. B. Esq.—Report and Collections of the Nova Scotia Historical Society, Vols. I. II. and III. 8vo. 1878–83.

United Service Institution, Royal—Journal, No. 122. 8vo. 1883.

United States Geological Survey—The Tertiary History of the Grand Cañon District. With Atlas. By C. E. Dutton. 4to and fol. 1882.
Second Annual Report, 1880–1. 4to. 1882.
Hayden's Twelfth and Final Report, 1878. 3 vols. 8vo. 1883.
Bulletin I: on Hypersthene Andesite. By W. Cross. 8vo. 1883.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1884: Heft 1. 4to.

Wild, Dr. H. (the Director)—Annalen des Physikalischen Central-Observatoriums, 1882, Theil I. 4to. 1883.
Repertorium für Meteorologie, Band VIII. 4to. 1883.

WEEKLY EVENING MEETING,

Friday, March 7, 1884.

HENRY POLLOCK, Esq. Manager, in the Chair.

C. VERNON BOYS, Esq. A.R.S.M.

Bicycles and Tricycles in Theory and in Practice.

WHEN I was honoured by the invitation to give this discourse on bicycles and tricycles, I felt that many might think the subject to be trivial, altogether unworthy of the attention of reasonable or scientific people, and totally unfit to be treated seriously before so highly cultured an audience as usually assembles in this Institution. On the other hand, I felt myself that this view was entirely a mistaken one, that the subject is one of real and growing importance, one of great scientific interest, and, above all, one of the most delightful to deal with that a lecturer could wish to have suggested to him.

It is quite unnecessary for me to bring forward statistics to show how great a hold this so-called new method of locomotion has taken upon people of all classes. The streets of London, the roads and lanes in all parts of the country testify more forcibly than any words of mine can do to what enormous numbers there are who now make use of cycles of one sort or another for pleasure or for the purposes of business.

Not only has the newly developing trade brought prosperity to towns whose manufactures were dying a natural death, but the requirements of cyclists have given rise to a series of minor industries themselves of great importance. Riders of bicycles and tricycles come along so silently that instruments of warning have been devised. There are bells that jingle, bells that ring, whistles, bugles, and a fiendish horn on which all the noises of the farmyard can be imitated, and which will utter anything from a gentle remonstrance to a wild, unearthly shriek. Lamps, tyres, saddles, seats, springs, &c., are made in unending variety. These form the endless subject of animated conversation in which the cyclist so frequently indulges. Cyclometers, or instruments for measuring the distance run, are also much used. Some show the number of revolutions made by the wheel, from which the distance can be found by a simple calculation, others indicate the distance in miles. There is on the table a home-made one of mine with a luminous face, which at the end of every mile gives the rider a word of encouragement; it now indicates that a mile is nearly complete: in another turn or two you will all hear it speak.

Cyclists have a literature of their own. There are about a dozen papers wholly or largely devoted to the sport. They can even insure themselves and their machines against injury by accident in a company of their own.

The greatest, and by far the most important, growth is the Cyclists' Touring Club, a gigantic club to which every right-minded rider in the country belongs. This club has done more to make touring practically enjoyable than could have been thought possible when it began its labours. Railway companies have, with few exceptions, consented to take cycles at a fixed and reasonable rate; in almost every town in the country an agreement has been made with the leading or at any rate a first class hotel, in virtue of which the touring member may be sure of meeting with courtesy and attention for himself, and with clean quarters and an intelligent groom for his horse, instead of finding himself, as hitherto, a strange being in a strange place, at the mercy of some indifferent or exorbitant landlord. In consequence of this, thousands now spend their holidays riding over and admiring the beauties of our own country, instead of being dragged with a party of tourists through the streets and buildings of a foreign town. Of the delightful nature of a cycling tour I can speak from grateful experience; last autumn alone I travelled nearly 1500 miles, meeting on my way with almost every variety of beauty that the scenery of this country affords. Wherever I went I felt the beneficial influence of the C.T.C., as the touring club is called. At all the hotels—our headquarters—at which I stopped I found the most sanguine wishes of the club amply fulfilled, our wants understood and provided for.

The C.T.C. have done a great service in providing us with a uniform which has been proved to be as near perfection as possible. They have also designed a lady's cycling dress, which can be seen in the library.

Though touring in the country is the perfection of our art, town riding has its advantages. I, in common with a fair number, ride daily to and from my work, no matter what the weather may be. Rain, snow, wind or hail, cycling affords the pleasantest means of crossing London. Instead of waiting in draughty railway stations, or catching cold outside or being stewed inside omnibuses, or of being smoked in the underground railway, we, the regular cyclists, look forward to our daily ride with pleasure; for the healthy exercise, the continuous necessity of watching the traffic and avoiding every approaching danger form between them a relief from mental worry or business anxiety which we alone can appreciate.

Of the dangers of the streets I have little to say: the regulation of the traffic by the police, and the consideration of drivers, though they are not in general too fond of us, make danger in the quarter from which it might be expected very remote. Our chief difficulty is due to the irregular and utterly unaccountable movements of pedestrians, whose carelessness keeps us in a continual state of anxiety.

There remains one point of the utmost importance, on which I would say a few words: I refer to the effect of cycling on our general health. About a year ago there appeared in the *Lancet* an article condemning in no measured terms the evils likely to result from the development of this new craze, in which, as far as I remember, it was stated that we are now sowing the seeds of a series of new diseases, the symptoms of which will only appear possibly in years to come. I would not for a moment question the accuracy of opinion held by any professional man: whether this is or is not the case I cannot tell; however, I may mention that the only symptoms which I have so far discovered in myself are an improved appetite, increased weight, and a general robustness to which I was formerly a perfect stranger.

Having, I trust, succeeded in showing that the advantages offered to riders are sufficient to account for the rapid development of cycling—that it is, in fact, no mere temporary craze—I shall now proceed to consider the theory and construction of the various machines at present known.

From the hobby-horse to the bone-shaker, and from the bone-shaker to the bicycle, the steps are so simple and obvious that it is quite unnecessary for me to trace them. It is also needless for me to describe the modern bicycle: every one must be familiar with it, every one must have seen the ridiculous zig-zag of the beginner, and have admired the graceful gliding of an accomplished rider. Of the theory of the balance little need be said; anything supported on a mere line, in unstable equilibrium, as it is called, must fall one way or the other. The machine and rider would of necessity capsize if some action of recovery were not possible. To whichever side the machine shows any inclination, to that side the rider instinctively directs it; by this means the tendency to fall to one side is balanced by the property of the rider to continue moving in a straight line and so to go over on the other side. This action of recovery is always overdone, so that a second turn in the opposite direction must follow. Hence the extraordinary path traced by the beginner. Even with the most skilful rider, though he appears to travel in a perfectly straight line, a slightly sinuous course is essential, as the highly characteristic track left on the road indicates. If anything should happen to check this slightly serpentine motion—as, for instance, occurs when the driving-wheel drops in the groove of a tram line—the balance at once becomes impossible, and the rider is compelled to dismount.

The extraordinary stability of the bicycle at a high speed depends largely on the gyroscopic action of the wheels. On the table is a top supported in a ring which is free to move how it pleases. So long as the top is spinning the ring is as rigid as a block; on stopping it the freedom of the support is at once apparent.

It is a marvel to many how anything so light, how anything so delicate, can carry the weight, or can travel at the speed so common, without utterly collapsing. The wheels especially attract attention. In a hoop no one part can be pushed in unless some other part can go out. A bicycle wheel is a hoop in which every part is prevented from going out by the tension of the spokes. To give the wheel lateral stability, the spokes are carried not to the centre, but to the two ends of the hub, thus lying on two cones. Such a wheel is abundantly strong in its own plane: it can withstand the jars and shocks of a bad road without a groan, but once subject it to a serious side strain, such as I can with ease put upon it with a jerk of my wrists, and the wheel will crumple up like an umbrella in a storm. Till this year there has been no change in the principle of construction, though in detail many improvements have been carried out and are largely adopted. By the use of hollow rims a stiffer and lighter wheel can be made; thick-ended, crossed, and laced spokes are employed, and other details modified. Essentially, however, the "spider" wheel as a structure is the same as it was when first introduced. Suddenly, two radical changes are presented to us. Mr. Otto, whose great work I shall describe in its proper place, has devised a wheel on a new system, in which the spokes that form the structure lie in the plane of the rim, in which position they are best able to withstand direct shocks. Such a wheel would be unstable, but requires very little to keep it true. Delicate spokes not screwed up very tight are therefore placed on either side, so that a side strain is met by the whole strength of the spokes on one side, which are not, as hitherto, weakened by the pull of the spokes on the other. On this system much narrower wheels can be made than was possible before. The other change, due to the same inventor, is still more striking. He has found, contrary to the opinion of every one, that wheels, either of his narrow type or of the usual form, can be made and will remain true when the spokes are made elastic by being bent into a wavy or slightly spiral form. If only these wheels will stand the test of time—and I see no reason why they should not—one of the greatest discomforts and probable causes of injury from which the cyclist suffers, the vibration and jolting due to a bad road, will have been removed.

The bearings in a bicycle are perhaps more to be admired than any single part. Instead of allowing the axle to slide round in its bearings, hard steel rollers or balls are introduced, so that the parts which are pressed together roll over and do not slide upon one another. Any one who has trodden on a roller or a marble must have found in a possibly unpleasant manner the great difference between rolling and sliding friction. I can now give for the first time the result of an experiment, only completed this morning, which shows the extraordinary perfection to which this class of work has attained. I have observed how much a new set of balls, which I obtained direct from the well-known maker Mr. Bown, has lost in weight in travelling 1000 miles in my machine. Every 200 miles I cleaned and weighed the balls with all the care and accuracy that the resources of a physical laboratory will permit. The set of twelve when new weighed 25·80400 grms., after 1000 miles they weighed 25·80088 grms., the loss being 3·12 milligrams, which is equal to $\frac{1}{20\cdot 8}$ grain, that is in running 1000 miles each ball lost $\frac{1}{250}$ grain. This corresponds to a wear of only $\frac{1}{158,000}$ inch off the surface. At this rate of wear—3·12 mgms. per 1000 miles—the balls would lose only $\frac{1}{34 \cdot 3}$ of their weight in travelling as far as from here to the moon.

The twelve balls, after the first 200 miles, each weighed in grammes as follows. The loss of each in running 600 miles is appended:—

Weight in grms.	Loss in 600 miles.	Weight in grms.	Loss in 600 miles.
2·16605	·00050	2·14725	·00020
2·16180	·00025	2·14725	·00020
2·15500	·00035	2·14700	·00020
2·15480	·00015	2·14500	·00020
2·15000	·00015	2·14280	·00025
2·14730	·00015	2·13875	·00020

I did not weigh each ball on the first and last occasion; however, the wonderfully uniform wear in the intermediate 600 miles speaks well for the equal hardness of the balls.

The wear of the dozen during each journey of 200 miles was as follows:—

Miles.	Wear in grms.
0 to 200	·00055
200 „ 400	·00070
400 „ 600	·00055
600 „ 800	·00075
800 „ 1000	·00062

I have given the results of these experiments at length, for I do not think that accurate and systematic observations of the kind have been made before.

We may consider, then, that the balls are practically indestructible. Knowing this, Mr. Trigwell has applied the ball bearing to the construction of the "head" of the bicycle, not so much with the view of diminishing the friction there, but of preventing wear in a place where any shake is highly objectionable. One of his ball heads is on the table.

The frame of the bicycle, consisting merely of the fork and backbone, is made of thin steel tube, the type of all that is light and strong. Indiarubber, besides being used for the tyres of all machines, has been worked into every part of the structure, to diminish, so far as is possible, that perpetual and wearying vibration of which all bicyclists so bitterly complain. The number of improvements in every detail is so great that any attempt to enumerate them is out of the question. Suffice it to say that the modern bicycle is the perfection of all that is perfect; as a machine for racing, as a machine for hurrying over good and level roads, nothing can approach it. Unfortunately, however, there is ever present danger, and danger of the most objectionable sort, for the most skilful rider knows too well that should he strike a stone of even an ordinary size, he must expect to be pitched over the handles and come with a crash to the ground.

It is true that in general no harm is done, but such a fall may bring any one to a sudden and horrible end.

Many have attempted, while still retaining the advantage of the bicycle, to make these involuntary headers impossible by modifying in some way its construction. One of the earliest attempts in this direction is well named the "Extraordinary." On it the rider is placed much further behind the main wheel, but can still employ his weight to advantage, as the treadles are placed below him and are connected by levers with the cranks. In another safety bicycle a third wheel is carried in front just above the ground, so as to resist at once any tendency to tilt forward. In another type much smaller wheels are employed, and the feet, now nearer the ground, are connected with the cranks by levers in the "Facile," or by a hanging pedal in the "Sun and Planet." There is a bicycle with two large wheels, one in front of the other, which two can ride, which should be both safe and rapid.

By far the most curious and utterly unintelligible of all machines of the bicycle type is Mr. Burstow's "Centre-cycle." So incomprehensible did this machine seem to me that I took the trouble one afternoon last week to ride to Horsham to see it in its native place. A careful examination has convinced me that it is not only correct in its design but that it is in many respects the most wonderful cycle at present made.

There is on the table a model Plympton skate. When this is level it runs straight, when inclined either way it wheels around in a manner that was so familiar a few years ago. The four wheels of the Centre-cycle are a counterpart of the four wheels of the skate: when the frame leans either way they turn in an appropriate manner, or, conversely, when they turn the machine leans in the proper direction. It might be thought that a thing with five wheels was more nearly allied to a tricycle than to a bicycle, but this is not so, for the Centre-cycle, when ridden skilfully, has rarely more than one wheel on the ground. The leaning to one side in turning a corner (tricycles, unfortunately, must remain upright) and the general action is essentially that of a bicycle. The great peculiarity of this machine is the power that the rider possesses of raising or lowering any wheel he pleases. Now that I have mounted it you will see that I can rest on one, three, four or five wheels, as I please. In consequence of this power of lifting the wheels, a rider can travel over an umbrella without touching it, lifting the wheels as they approach and dropping them as they pass, after the manner of a caterpillar.

Till a few years ago the bicycle was the only velocipede which was worthy of the name. Inventive genius and mechanical skill have given rise to a series of machines on three wheels on which any one can at once sit at ease, and which require but little skill in their management. Men who do not care to risk their necks at the giddy height of the bicyclist, ladies, to whom the ordinary bicycle presents difficulties which they cannot well surmount, each find in the tricycle the means of obtaining healthy and pleasant exercise and of enjoying to a certain extent the advantages which the bicycle affords. Thanks to the perfection of the modern tricycle, cycling has become one of the most popular institutions of the day.

Whatever difficulty I may have had in doing justice to the bicycle, the corresponding difficulty in the case of tricycles is far greater. The number of makers and the variety of their work is so great that it would be sheer madness on my part to attempt to describe all that has been done. Those who wish to see the great variety of detail which chiefly constitutes the difference between one make and another must go to one of the exhibitions of these things which are now so common.

All I shall attempt will be an explanation of the leading principles which are involved in the design of a tricycle. For this purpose it will be necessary for me to mention occasionally some particular machine; but, in justice to the hundreds to which I cannot even refer, I wish it to be understood that those named, though typical, are not of necessity better than any other.

It is first necessary to know what combinations of three wheels will and what will not roll freely round a curve. The few possible arrangements determine the general forms which a tricycle can take. A wheel can only travel in its own direction: no side motion is possible without the application of considerable force, entailing strain and friction of a most injurious kind. In any combination, then, of three wheels each must be able, in spite of the united action of the other two, to move in its own direction. There is on the table a model in which the three wheels can take every possible position. To begin with, two large ones are placed opposite but independent of one another and parallel, and a small one parallel to the others is mounted between them at one end. This arrangement rolls along in a straight line with perfect freedom; on twisting the plane of the third wheel it is also free to roll round a curve whether the little wheel is before or behind. If I shift the position of one of the large wheels so that though still parallel to, it is no longer opposite, the other, then, though they can freely move in a straight line, they can by no possibility be induced to roll round a curve. It is clear, then, that two wheels which are parallel cannot be employed in a tricycle unless they are opposite one another. The only class of people who frequently appear to be familiar with this fact are nursemaids, who always tip up the front of a perambulator in turning a corner. If one wheel is in front of and another behind a third, the combination can only freely turn a corner when the front and rear wheels are turned to proportionate extents in opposite directions. The model is so arranged; now, if either of the little wheels is not turned to exactly the right amount they can no longer roll, they can only be dragged round a curve. It is not sufficient that two parallel wheels should be opposite one another: they must be able to turn at different speeds. I have now the two large wheels keyed on the same axle, so that they must of necessity turn together; this combination is ready enough to go straight, but no amount of encouragement by the steering wheel will induce it to go in any other direction.

Bearing these facts in mind it will not be difficult to follow the development of the tricycle. It would seem impossible in the first arrangement (that with two wheels opposite one another, and a third or steering wheel before or behind between them) to drive both sides, for the wheels must be able to turn at different speeds; let, therefore, one be free to go as it pleases, if the other only is driven, we have at once a very common form of tricycle in which one wheel drives, one steers, and one is idle. Machines of this class have many defects. The feeble steering power, combined with their unsymmetrical driving, render them altogether untrustworthy. If much power is applied to the driver, which can only have its share of the weight upon it, it slips on the ground: if the machine is quickly stopped, owing to the small weight on the steering wheel it is apt to swing round and upset; nevertheless, those who are content with pottering about on our wood pavement and gravel roads find this class of machine answer their purpose, and owing to its cheapness and simplicity they do not care to get a better. The second arrangement of the model, in which riders must have recognised the "Coventry rotary," is free from most of the defects of the form just described: there is more weight on the driver but not enough to prevent its being made to slip round; there are two steering wheels a long way apart with plenty of weight upon them, so that the guiding power in this type of tricycle is all that can be desired.

Let me now return to the first arrangement, in which two parallel wheels are opposite one another. If by any possibility both wheels could be driven and yet be free to go at different speeds, then, there being so large a weight on the drivers, they could not be made to slip; the driving being symmetrical most of the twisting strain would be taken off the steering wheel and still the machine would be capable of rolling round a curve with perfect freedom.

All the methods of solving the problem of double driving come under two heads, one depending on the action of a clutch and the other on differential or balance gear.

The clutch action being the simplest, I shall describe that first. In going round a corner the inner wheel must lag behind or the outer wheel must run ahead of the other; as either wheel must be inner or outer, according to the direction of the curve, each must be able to lag behind or each must be able to run ahead. If both were able to lag behind, the machine could not be driven forward and it would be of little use; if both were able to run ahead, the machine could not be driven backwards, a matter of small importance. There is on the table a large working model showing how a four-sided wheel is free to revolve in a ring, but is instantly seized when turned the other way owing to a jambing action of one or more of four rollers. The four-sided wheel, then, can be employed to drive the ring one way but not the other. One of these "clutches" or "friction grips" is placed at each end of the crank-shaft in the "Cheylesmore" tricycle, and a chain round the ring of each drives the corresponding wheel. The machine named is a rear steerer. The clutch is also employed in some front steerers.

The other method of double-driving depends on the use of the well-known gear of three bevil wheels, or of some equivalent mechanism. If the axle of the middle of the three wheels is turned round the common axle of the other two, the applied force is divided between those two wheels, yet the pair are free to move relatively. Let, then, the chain drive a wheel carrying the middle bevil and let the side bevils be connected with the two drivers; whatever happens, the power of the rider will be equally divided between them, yet the machine will be free to roll round a curve.

There are a great number of devices which are exactly equivalent to this, the simplest of all which is known as Starley's gear. There is on the table a beautiful model of the gear used in the Sparkbrook tricycle which has been lent me by the makers of that machine, Bown's differential gear, and some others; but time will not allow me to describe them. There is one gear, however, which presents many peculiarities, which I have devised, and which may be of interest. A large working model is on the table. Between the conical edges of two wheels which are connected to the drivers, lie a series of balls outside which is a ring with sloping recesses. If the ring is turned by a chain or otherwise, the balls jamb in the recesses as the rollers do in the clutch gear. Nevertheless, they are free to turn about a radial axis, and so allow the two driven cone wheels independent motion. The bursting strain on the ring and the side thrust on the cones acting on another set of rolling balls, balance one another. With this gear the rider can cause the balls to jamb one way or both ways, and so have or avoid the "free pedal" as he pleases.

Having spoken of the differential gear and the clutch, I had better show the comparative advantages and disadvantages of the two methods of double driving. With the differential gear the same force is always applied to each wheel, so in turning a corner the outer one, which travels furthest, has most work expended upon it (work = force × distance). In this respect the differential gear is superior. On the other hand, when one wheel meets with much resistance from mud or stones and the other with hardly any, the latter has still half the strength of the rider spent upon it, which is clearly a mistake. With a clutch-driven machine running straight, the wheels take such a share of the rider's power as is proportional to the resistance they individually meet. When the machine is describing a curve—that is, generally—only the inner wheel is driven and the machine is for the time only a single driver with the driver on the wrong side.

In almost all good designs of front-steering tricycles, the power applied to the cranks is transmitted to a differential gear by a chain. The crank and connecting rod have also been used to transmit the power, but then the clutch is necessary.

There is, however, another type of tricycle in which the use of cranks is avoided, among which may be mentioned the "Omnicycle," the "Merlin," and that highly ingenious machine the "Oarsman" tricycle. On the table there is the Omnicycle gear. In all these the power is applied direct to the circumference of a wheel or sector, and so dead points are avoided, which is a point in their favour when meeting with much resistance. On the other hand, the sudden starting and stopping of the feet at every stroke in the two former machines and of the body in the latter makes this type utterly unsuitable for obtaining anything more than a moderate speed. In the Omnicycle ingenious expanding drums are employed so that the power may be applied with different degrees of leverage according to circumstances.

There remains one type of tricycle which for rapid running surpasses many: I refer to what is known as the Humber pattern. So excellent is this form in this respect that the leading manufacturers have, by turning out machines on the same lines, paid the original makers a compliment which is not altogether appreciated. This pattern departs less from the ordinary bicycle than any other; it is one, in fact, in which, instead of one, there are two great wheels, giving width to the machine, between which the power is divided by the usual differential gear.

I must now describe some devices which are attracting much attention at the present time, the speed and power gears. Let us suppose there are two machines with wheels of different sizes, but in other respects alike. Then each turn will take the larger-wheeled machine further than the smaller. In going up a hill the larger wheel will take its machine up a greater height than the other in one revolution, which involves more work and therefore more strength. If on the large wheel the chain pulley were increased in size, then for the same speed of the treadles it would not turn so quickly: it would not take the machine so far up the hill as before: it would, in fact, be equivalent to a smaller wheel, so that less strength than before would be necessary. This diminution of speed, though of great advantage when climbing a hill, is the reverse on the level, for there very rapid pedalling would be necessary to maintain even a moderate speed. To obtain the advantage of high wheels or high gearing on the level, and at the same time low wheels or low gearing on the hills, some highly ingenious devices are employed. On the table is a well-known example, the "Crypto-dynamic," which by a simple movement changes the relative speed of wheel and treadle. Time will not permit me to describe the details of this arrangement, but it contains an epicyclic gear, which is or is not in action, according as the rider desires power or speed. There are several other devices having the same object, some depending on an epicyclic gear in a pulley, others on the use of two chains, only one of which is active at a time. These arrangements have the further advantage of enabling the rider to disconnect the treadles from the wheels whenever he pleases. Tricycles on which two, three, or a whole family can go out for a ride together involve few new principles, and I shall not, for this reason, have a word to say about them.

There remains one machine forming a class by itself more distinct from all others than they are from one another. It is not a bicycle in the ordinary sense of the word; it is not a tricycle, for it has only two wheels. This machine is from a scientific and therefore from your point of view more to be admired than any other. It is called, after its inventor, the "Otto." The Otto bicycle and the Otto gas-engine will be lasting memorials to the ingenuity of the brothers who invented them.

No machine appears so simple but is so difficult to understand as this. Tricyclists who have been in the habit of managing any machine at once are surprised to find in this something which is utterly beyond them. They cannot sit upon it for an instant, for so soon as they are let alone it politely turns them off. When at length, after much coaxing, they can induce it to let them remain upon it, they find it goes the way they do not want. Riding the Otto, like any other accomplishment, must be learnt. Some seem at home on it in half an hour, others take a week or more. It is not surprising that that quick perception in which ladies have so much the advantage of men enables them to quickly overcome the apparently insurmountable difficulties which this machine presents to the beginner.

The rider, when seated, is above the axle of two large equal wheels, being then apparently in unstable equilibrium: he would of necessity fall forwards or backwards if some movement of recovery were not possible. The Otto rider maintains his balance in the same way as the pedestrian. If he is too far forward, pressure on the front foot will push him back; if too backward in position, pressure on the rear foot will urge him forward. That this must be so is clear, for whatever turning power he applies to the wheels, action and reaction being equal and opposite, they will produce an equal turning effect upon him. The steering of this machine is quite peculiar. In the ordinary way both wheels are driven by steel bands at the same speed: so long as this is the case the Otto of necessity runs straight ahead. When the rider desires to turn he loosens one of the bands, which causes the corresponding wheel to be free; if then he touches it with the brake or drives the other wheel on, it will lag behind and the machine will turn. It is even possible to make one wheel go forwards and one backwards at the same time, when the machine will spin like a top within a circle a yard in diameter.

There being no third wheel the whole weight is on the driver, the whole weight is on the steerers; the frame, which is free to swing, compels the rider to take that position which is most advantageous, making him upright when climbing a hill and comfortably seated when on the level. Owing to a curious oscillation of the frame which occurs in hill-climbing the dead points are eliminated, so the rider need not waste his strength at a position where labour is of no avail.

Though it has been impossible for me to do more than indicate in the most imperfect manner how numerous and beautiful are the principles and devices employed in the construction of cycles, I trust I have disappointed those who were shocked and horrified that so trivial a subject should be treated seriously in this Institution.

[C. V. B.]

WEEKLY EVENING MEETING,

Friday, March 14, 1884.

Sir Frederick Pollock, Bart. M.A. Manager and Vice-President, in the Chair.

J. N. Langley, Esq. M.A. F.R.S.

The Physiological Aspect of Mesmerism.

Scattered about in the literature of the seventeenth and eighteenth centuries are many records of the cure of divers human maladies in simple and mysterious-seeming ways.

Valentin Greaterakes, in Charles II.'s reign, was, we are told, "famous for curing various diseases and distempers by a stroak of the hand only." His power, he thought, was a special gift from heaven. Many people, however, were not slow to say that he had dealings with the devil. In some cases wonders were wrought by touching the affected parts of the patient with a magnet. Maxwell, who in 1679 published a short treatise on magnetic medicine, attributed the cures brought about by this, and by some other unusual forms of medical practice, to the accumulation of a subtile fluid in the body of the patient. This subtile fluid was diffused through all things in nature; a fortunate few amongst men had an inborn power of controlling its distribution. Such men could cure all diseases; they could indeed, he says, by adding to their own proper quantum of fluid, make themselves live for ever, were not the influence of the stars adverse.

In 1775 the theory of animal magnetism was put forward in Vienna by Friedrich Anton Mesmer. Neither his theories nor his facts differ very greatly from those of some of his predecessors. There exists, he said, in nature a universal fluid; in virtue of this, the human body possesses "properties analogous to those of a magnet; there are to be distinguished in it poles equally different and opposite, which may even be communicated, changed, destroyed, and restored; even the phenomenon of inclination is observed therein." By means of this magnetic fluid all the maladies of man could be healed. A few years later Mesmer left Vienna for Paris. At first he magnetised his patients by gazing steadily at them, or by means of "passes"; but as patients became more numerous, he brought them into a proper magnetic condition by other methods, often of a very fantastic nature. The patients did not, when magnetised, all show the same symptoms; some passed into a heavy sleep, some became insensible to touch, or even to stimuli ordinarily painful; some became cataleptic, some were seized with local or general convulsions. This last condition was called a crisis, and was the triumph of the mesmeriser, the moment when the disease was considered to be forcibly expelled from the system. Now-a-days it is the last state a physician would care to produce in a patient.

For a time Mesmer's success was enormous. His admirers subscribed for him a sum of nearly 350,000 francs, receiving in return details as to the method of magnetisation. In Paris the belief in the power of Mesmer to cure diseases soon waned; but by this time he had made a stir in the world, and had drawn attention to a number of facts which were either only locally known, or largely disregarded.

Mesmer devoted himself chiefly to curing patients, and it must be added, to receiving fees; but about ten years after the time of his coming to Paris it was found that a state resembling somnambulism, or sleep-walking, could be produced in some persons by magnetising them. This gave a stimulus to the investigation of what I may call the magical side of the phenomena. This magical side had always been present, but in the height of Mesmer's power had not been much regarded. Of the magic of animal magnetism I will say one word more presently.

The term animal magnetism lingered long, but has now happily fallen into disuse, either mesmerism or hypnotism being used in its stead. "Hypnotism" we owe to Dr. Braid of Manchester, who, from 1841 to the time of his death in 1860, subjected all the phenomena said to be produced in the magnetic state to a searching investigation. Braid is the founder of mesmerism in its scientific aspect. Hypnotism and mesmerism, as commonly used now, are synonymous terms; it would be advantageous, I think, if we could make a distinction between them. We might, for example, use the term hypnotism to embrace all those phenomena which are proven, and the term mesmerism to embrace all those phenomena which are not proven. Mesmerism would then mean what I have called its magical side and would embrace those phenomena which are sometimes called the *higher* phenomena of mesmerism. These are of various kinds. It is said, for instance, that one person can, at any time he wishes, mesmerise another who is at a distance, and who is in perfect ignorance of the intentions of the mesmeriser; that a mesmerised person can perceive the thoughts and sensations of the mesmeriser, without receiving any indications from the known organs of sense; that a clairvoyant can see with parts of the body other than the eyes, for example with the back of the head, or with the pit of the stomach; that a clairvoyant can describe places and persons which he has never read of, or heard of, or seen. Those observers who have done most to elucidate the subject, such as Braid, have failed to observe any of these and other similar higher phenomena. They are unproven. It would be convenient, I say, to include such phenomena only, under the heading of mesmerism; but this I cannot yet venture to do. The facts I have to mention I shall call those of hypnotism or mesmerism indifferently. The magical side of the subject may, I think, at present be fairly left out of account.

Primarily, the hypnotic or mesmeric state is one in which the will is partially or wholly paralysed by certain sensory impressions; but there is no distinct line of demarcation between this and various other conditions, such as occur in sleep, somnambulism, and in some diseases of the central nervous system, such as hysteria. In each there is a typical state, but between them are many transition states.

Before discussing the mesmeric condition, I must say one or two words about the action of the central nervous system. I trust you will forgive me if, as very well may be the case, you find that part of what I say seems too simple to need saying, and part too complex and uncertain to be said without reservation. The one for the sake of clearness must needs be stated; the other for the sake of brevity must needs be dogmatic.

Here is a diagram of the brain and of the spinal cord of the frog. In this, all the chief structures of the brain of man are represented. For my present purpose it is only necessary to distinguish three divisions.

First there is the spinal cord. If a frog be decapitated, the brain is of course removed and the spinal cord is the only part of the central nervous system left. Yet if any part of the body of the brainless frog be gently stimulated, a particular movement results—a reflex action is produced. If, for instance, the right hind leg is gently pinched, this leg and this only is kicked out; if the left fore leg is gently pinched, this and this only is moved. Diagrammatically we may represent any one of these movements as being brought about in the following way. Pinching the skin stimulates the nerve endings of a sensory nerve, so that a nerve impulse—analogous to, but not identical with, an electric current passing along a wire—travels up the nerve to a sensory nerve cell in the spinal cord. In this nerve cell certain changes take place which result in an impulse being sent along another nerve to a motor nerve cell in the spinal cord. This is, in consequence, stimulated to activity and sends out a third impulse along a motor nerve to a muscle. The muscle then contracts, and the limb is moved.

If the brainless frog be pinched somewhat sharply, the movements which result are more extensive than when it is gently pinched, a spasm of the whole body may result. Referring to the diagram, we may represent this in the following way. The sensory cell being more strongly affected, sends out impulses to a number of other sensory cells on the opposite side of the spinal cord, and above and below it; these send impulses to their motor centres, and thus a more or less widely-spread movement results. This spreading out of impulses from the part immediately affected is called the irradiation of exciting impulses. When any part of the skin is stimulated, many sensory and many motor cells are affected; a collection of cells serving a common purpose is called a nerve centre. The spinal cord, then, consists of a collection of nerve centres. By appropriate stimulation, any one, or all of these nerve centres can be set in activity.

The second division of the central nervous system is the posterior part of the brain—the brain minus the cortex of the cerebral hemispheres. This, like the spinal cord, consists of a collection of nerve centres, but the function of these nerve centres is much more complex than that of the centres of the spinal cord. A stimulus to the skin, which, when the spinal cord is the only part of the central nervous system left, will produce either a local movement or no movement at all, will, when the posterior part of the brain is also present, produce a general co-ordinated movement such as occurs in walking, jumping, swimming. In fact, all the co-ordinated movements of which the body is capable can be brought about by the activity of one or more of the lower centres of the brain. Moreover, these centres can be set in action by events which have no effect when the spinal cord only is present. Here a flash of light or a sudden noise sets in activity a nerve centre in a manner strictly comparable to the way in which a pinch applied to the foot sets in activity a nerve centre in the spinal cord; and just as in the spinal cord the active sensory centre may excite to activity a motor centre, and this may cause the foot to be moved, so in the lower centres of the brain the activity of the visual or auditory centre may excite to activity a motor centre and lead to a complicated movement such as shrinking or jumping. A frog with these two divisions only of the central nervous system does nothing of itself; it is without will and consciousness, in the same way that the frog with a spinal cord only, is without will and consciousness; it is a complicated machine, any part of which can be put in action by using the proper means.

The last division of the central nervous system is the *cortex* of the cerebral hemispheres. This part of the brain is concerned with ideas, with will, and with consciousness in the sense in which that term is usually employed, that is, speaking generally, it is concerned with the higher psychical functions.* In saying that this part of the brain is *concerned* with the higher psychical functions, I mean that every higher psychical act is accompanied by some definite change in the cortex of the cerebral hemisphere. I mean that every emotion, every idea, every effort of will is accompanied by an activity of nerve cells in this part of the brain and that this activity is comparable to the activity which takes place in definite cells of the spinal cord when a leg or arm of a brainless frog is pinched.

Here we touch the much disputed question of the localisation of the functions of the brain. Roughly speaking, this question is whether there are nerve centres in the cortex corresponding to those which exist in the rest of the brain and in the spinal cord:—

---

* It is not possible within the limits of this lecture to give the reservations that would be necessary in a full discussion of the subject.

whether, for example, visual sensation and ideas are accompanied by an activity of one part of the cortex, and auditory sensation and ideas are accompanied by an activity of a different part of the cortex; or whether visual and auditory sensation and ideas may occur in any part of the cortex, the mode of activity of the cells being different in the two cases.

Happily, it is not necessary to enter into this question in order to gain a fair idea of the chief features of mesmerism. The idea which we gain lacks no doubt definiteness in detail, and we must be prepared to express it in different language according as we find later, that the cortex of the cerebral hemispheres consists of one nerve centre with many functions, or of many nerve centres with different functions, or again as we find—and this is most probable—that the truth is between these two extreme theories.

But whilst we may put in the background the question of localisation of function for the cortex of the brain, we must linger a little to consider its mode of action. I will take a particular instance.

The changes which occur in the retina of the eye when rays of light from an external object fall upon it give rise to nervous impulses which eventually produce in the cortex of the brain a certain activity; this activity leads to our forming an idea of the object. Now in some cases the formation of the idea is all that takes place: often, however, impulses are sent out from the active cells of the cortex to a motor centre in the lower part of the brain, and a movement is made. This is a reflex action from the cerebral cortex. Here the active sensory centre excites a motor centre, just as happens in the spinal cord of a frog the leg of which is pinched. Our actions are often of this nature, though in many cases of course it is very difficult to say how far the will is exercised in the action. If you give a child a sweetmeat, the child sometimes no doubt deliberates what to do with it; in others the rapid transference of the sweetmeat to the mouth seems to be simply a reflex action entirely independent of any effort of will, though accompanied by consciousness.

Dr. Carpenter has introduced the useful term unconscious cerebration into physiological-psychology. By this is meant that the cortex may be active without our knowing anything about it. An instance which Dr. Carpenter gives, is that of trying to remember a name which for the moment we have forgotten, in such cases it is often best to give up consciously thinking, but the fundamental activities in the brain which accompany thinking go on nevertheless, so that presently without farther conscious effort, the name is remembered, is as it were thrown up into consciousness.

I said a moment ago that reflex actions not infrequently occur in which a conscious idea forms part of the reflex chain. But consciousness is not necessary to the reflex action; that is, the changes in the cortex which are the physical basis of the idea may be carried out without giving rise to consciousness. Here we want a term to imply that state in which everything necessary for an idea is present except consciousness. Sometimes this is called an "unconscious idea," which would be convenient enough but that "idea" is generally taken to imply consciousness. It is an act of unconscious cerebration.

Reflex actions in which an unconscious cerebration forms part of the chain occur to all of us. Some time ago, whilst walking up and down the laboratory at Cambridge thinking intently on the result of an experiment, I noticed that the pipe which I had been smoking had gone out. Making up my mind to light it again, I walked to the place where the matches were kept, which happened to be close to a water-tap. As I went I began thinking again of my experiment. In a moment or two I was disturbed by a rush of cold water over my hand. I found that I had turned the water-tap, and let the stream of water run full into the bowl of my pipe. This was a reflex action from the cerebral cortex. The sight of the tap had given rise to what for this once I will call an unconscious idea, which had led to the somewhat complex movements of turning the tap and collecting the water in the pipe-bowl.

The central nervous system consists, then, of a vast number of nerve centres, each of which can be set in activity by an appropriate nerve impulse reaching it either by a peripheral nerve or from some other nerve centre. The action of these nerve centres is normally controlled by the will.

Here, at last, we come to mesmerism; the primary point in mesmerism is the paralysis of the will; the nervous system is then out of the control of the subject, whether animal or man, and by appropriate stimulation, any one or more of his nerve centres can be set in activity. I shall consider first the behaviour of the lower animals when mesmerised: in these the phenomena, as far as at present observed, are much simpler than they are in man. If a frog be turned over on its back, it at once regains its normal position; if, however, it be prevented from doing so, and its struggles are for a short time gently suppressed, it becomes hypnotised. Then, although it be left at liberty to regain its normal position, it will not attempt to do so. Apart from the movements it makes in breathing, it lies motionless. If it has been held for a short time only, the hypnotic state does not last long, usually from one to five or ten minutes; but, if the movements it makes, say at the end of one minute, of five minutes, and so on, are suppressed, it will not infrequently happen that the frog will then stay without farther movement for a considerable time, sometimes even for many hours. During the first part of this time a slight pinch, a sudden flash of light, or a loud noise, will usually cause it to turn over and sit up in its normal manner. For a moment or two it looks a little dull and confused, but rapidly regains its normal activity. During the latter part of this time it responds less and less to external stimuli. Reflex actions are less readily obtained, or may not be produced at all by stimuli ordinarily effective. Within certain limits, the longer the frog remains hypnotised, the more marked becomes its general insensibility, the decrease in reaction being earliest distinct in the centres of special sense. When it is in this state, it may be propped up against a support with its legs crossed under it, or placed so that it rests on its head, or placed on its side with its legs arranged in this or that fashion, without offering the least resistance. Strong stimuli, or certain apparently lesser ones, for example a dash of water, cause it to recover its position slowly; it then usually sits for several minutes motionless, and only after some time regains its normal sensitiveness and activity. I show you here a frog in the early hypnotic state.

I have spoken of the frog as being hypnotised or mesmerised. Let us consider what is meant by this. I think it is obvious that the animal does not remain passive from any astuteness on its part; it is incredible that the frog, finding its efforts to escape ineffective, should make up its mind to remain quiet, and should, although at liberty to move, stay still for hours, becoming more and more determined as time goes on to take no notice of noises, of flashes of light, and of pinching of its skin. On the contrary, it is, I think, obvious that in some way its will has become paralysed. In order to attempt to explain how this is brought about, we must consider another aspect of reflex action, an aspect which is very little understood.

You remember that a brainless frog will, when its leg is gently pinched, kick out the leg; but if just previously some other part of the body has also been pinched, one of two opposite things may take place: the leg may be kicked out more quickly and vigorously, or it may not be kicked out at all. In both cases the nerve centre involved in producing the movement of the leg receives an additional impulse from another nerve centre, but in one case the additional impulse increases the activity of the nerve centre involved in the reflex action, in the other case it annuls this activity—there is, to use the physiological term, an *inhibition* of the "reflex" nerve centre.

To take another instance, a frog without its cerebral hemispheres, but with the rest of its nervous system, will croak when its sides are gently touched; but if at the moment of touching it, its leg be pinched, it moves or jumps, but does not croak. Here the motor centre which causes the movements of the muscles in croaking, receives nervous impulses from two sensory centres; one of these being set in activity by touching the sides of the frog, the other from pinching its leg. The impulses from the former tend to make the motor centre active, and so produce a croak; but the exciting effect of these impulses is annulled by the impulses coming from the latter centre; in other words the nerve centre involved in croaking is inhibited. Inhibition by impulses proceeding from the cortex of the brain occurs every day of our lives. The "will" is perpetually being brought into play to inhibit some nerve centre or other. For example, you may be on the verge of yawning, when it suddenly occurs to you that it will be better not to do so; you suppress the yawn without moving a muscle. What happens is this. An inhibitory nerve impulse is sent from the cortex, and puts a stop to the indiscreet activity of a nerve centre elsewhere in the brain. Further, when the cortex is set in activity in a particular way by one impulse, another impulse reaching it may inhibit the first activity, or, in terms of the localisation theory, one nerve centre in the cortex may send out inhibitory impulses to any other nerve centre of the cortex.

I need not farther multiply instances of inhibition. I wish, however, to lay stress on this, that it is highly probable that impulses travelling from any peripheral nerve-ending to a nerve centre, or from any one nerve centre to any other, may, under certain circumstances, diminish or annul the functional activity of the nerve centre, that is, may inhibit it. And there is equal reason to believe that, under certain other circumstances, the effect produced will not be inhibition, but an increase of activity of the centre. The exact conditions which determine whether one effect or the other takes place, have not as yet been made out. For the present the facts must suffice us. We may now return to the mesmerised frog.

Whatever the will may be, its action is accompanied by a certain activity of the cortex of the brain; if this activity is prevented from taking place, the will can no longer act. From the physiological standpoint, then, the mesmerised frog lies motionless because an inhibition of a particular activity of the nerve cells of the cortex has taken place. We may distinguish two chief causes of this inhibition.

The tactile stimuli sent to the central nervous system when the frog lies on its back are obviously different from those sent when the frog is in its normal position. The unusual nerve impulses travelling from the skin in the unusual position of the frog are inhibitory nerve impulses. There is reason to believe that they act first on some lower centre of the brain, and that from this, impulses are sent which diminish or annul the activity of the cortical nerve cells which is necessary for the exercise of will.

The second chief cause of inhibition is in the cortex itself. Handling the frog in the way which is done when it is mesmerised, produces a certain emotional condition which we may call fright. But when the animal is frightened, the nerve cells of the cortex are set in activity in a special manner. This mode of activity inhibits other modes of activity, and the will is paralysed.* We cannot at present, I think, put in any more definite form the effect of one state of the cortex of the brain upon its other possible states. We do not know enough of the relations of the cortex of the brain to the psychical functions to say more. In some cases fright seems to play a very small part, if any, in producing the effect. That it is not an essential factor is, to some extent, confirmed by the fact that a frog without the cerebral hemispheres can be easily mesmerised; it is difficult to conceive of the animal in this state being very much frightened.

---

* The term "paralysis of the will" is here used to include the state in which there is an effort of will, but in which the effort is not followed by a despatch of nervous impulses from the cerebral hemispheres to the lower nervous centres.

It will be remembered that reflex action from all parts of the body is diminished in the mesmerised frog. After a time, then, there is a marked inhibition of activity of the whole nervous system. Now in the brainless frog placed on its back there is no such diminution of reflex action; hence in the intact hypnotised frog the spinal cord must be inhibited by impulses coming from the brain; from which we may conclude that centres inhibited in their own proper action, nevertheless send out inhibitory impulses to other centres. There appears then to be an irradiation of inhibitory impulses, just as we have seen that there is an irradiation of exciting impulses.

There are two other features in the hypnotised frog which I must mention, although time will not allow me to discuss them. It sometimes happens that soon after a frog has been hypnotised, reflex actions, instead of being more difficult to obtain than normally, are obtained more easily. Preceding the condition of reflex decrease there is a condition of reflex increase. Further, it sometimes happens that the hypnotised frog, instead of lying with its muscles flaccid, passes into a cataleptic state, so that its limbs tend to remain in any condition in which they are placed. Both the condition of reflex increase and that of catalepsy are more marked in man.

Before passing to mesmerism in man I will show you two other instances of hypnotism in the lower animals. The alligator which you see here behaves very much like the frog. It has, however, less tendency to become cataleptic. After a brief struggle, it becomes quiescent and its limbs slowly relax; its mouth may then be opened, and a cork placed between its teeth, without giving rise to any voluntary movement on its part. It may be kept for a considerable time in this limp condition by gently stroking the skin close to its eyes.

So far as I have observed, the hypnotic condition in birds and in lower mammals is not capable of any great development. It may last ten minutes, but rarely longer. In these animals, too, the emotional condition is probably the chief factor in producing the inhibition. Of impulses from peripheral sense organs, tactile impulses seem to be most effective in the lower mammals, as in the rabbit and guinea-pig, and visual impulses in the bird. The pigeon which I have here, remains longest quiescent when, after it has been held for a minute or two, I bring my hand slowly up and down over its head.

In man the phenomena of mesmerism are of a very much more striking character than they are in the lower animals. Speaking generally, this seems to be due to a greater interdependence of the various parts of the nervous system in the lower animals. In these, when any one centre is stirred up by exciting impulses, an irradiation of exciting impulses is apt to take place to all other centres, and the mesmeric state is in consequence apt to be broken. And on the other hand, when a centre is inhibited, an irradiation of inhibitory impulses is apt to take place, and the whole nervous system is in consequence apt to be inhibited. Hence the activity or suppression of activity of particular parts of the central nervous system, which forms so conspicuous a feature of mesmerism in man, can be only partially produced in the lower vertebrates. Even in man there is very considerable difference, in different individuals, in the ease with which particular nerve centres can be excited or inhibited without other centres being similarly affected. But apart from this the fundamental features are the same, whether a man or a frog be mesmerised. The primary point is, as I have said, the paralysis of the will, that is, the inhibition of a certain activity of the nerve cells of the cortex of the cerebrum.

In man, as in the frog, this inhibition may be brought about either by impulses proceeding from the peripheral organs of sense, or by impulses originating in the cortex itself. Of the former class, tactile and visual impulses are most effective, although the mesmeric state may be produced by auditory and probably by other impulses. A man may, then, be mesmerised by passing the hands over or close to the skin, or by making him look steadily at an object, or listen intently to a sound.

Whether the inhibitory impulses so set up produce inhibition or not, depends upon the condition of the whole of the nervous system. The effect of the inhibitory impulses may be counteracted by exciting impulses coming from other parts of the central nervous system. In many people the exciting impulses are always sufficiently strong to overpower the inhibitory ones, and such people cannot be mesmerised. In others, the inhibitory impulses must be kept up for a long time, and repeated on successive days, before they acquire sufficient force to overcome the exciting ones. Such people are mesmerised with great difficulty.

The great majority of people cannot be mesmerised unless they consent to fix their attention on some particular object. This fixing of the attention, speaking generally, seems to be a voluntary exclusion of exciting impulses, leaving thus the inhibitory ones an open field. Idiots, who, on account of the lack of co-ordination of their nerve centres, cannot fix their attention for any length of time on any one object, cannot as far as I know be mesmerised. Now this, now that part of the brain becomes active, and exciting impulses are sent out which overpower the inhibitory ones.* Inhibition from impulses arising in the cortex itself are rare unless the patient has been previously mesmerised. Some such cases, however, do occur. But in people who have been previously mesmerised inhibition in this manner is of not unfrequent occurrence; within limits, the more often the changes in the cells accompanying inhibition have been produced, the easier they are to reproduce. Those who have often been mesmerised may fall again into this condition at any moment, if the idea crosses their minds that they are expected to be mesmerised.

---

* It is said that some persons, whilst they are sleeping, can be brought by means of passes into the mesmeric state. It would be interesting to observe if this can also be done with insane people.

Thus if a sensitive subject be told that the day after to-morrow at half-past nine he will be mesmerised, nothing more need be done; the day after to-morrow at half-past nine he will remember it, and in so doing will mesmerise himself.

An instance sent by M. Richer to Dr. Hake Tuke, presents, it seems to me, an example of inhibition from the cortex which is of a somewhat different class, and more allied to that which occurs in birds and lower mammals. A patient was suspected of stealing some photographs from the hospital, a charge which she indignantly denied. One morning M. Richer found this patient with her hand in the drawer containing the photographs, having already transferred some of them to her pocket. There she remained motionless. She had been mesmerised by the sound of a gong struck in an adjoining ward. Here, probably, the changes in the cortex accompanying the emotion which was aroused by the sudden sound at the moment when she was committing the theft, produced a widespread inhibition—she was instantaneously mesmerised.

I will show you the method of mesmerising which is, perhaps, on the whole, most effective; it is very nearly that described by Braid. I have not time to attempt a mesmeric experiment to-night, it is the method only which I wish to show you. With one hand a bright object, such as this facetted piece of glass, is held thus, eight to twelve inches from the subject, so that there is a considerable convergence of the eyes, and rather above the level of the eyes, so that he is obliged to look upwards. The subject is told to look steadily at the piece of glass, and to keep his whole attention fixed upon it. This position is kept up for five to ten minutes; during this time the pupils will probably dilate considerably, often assuming a slight rhythmic contraction and dilation; when this is the case the free hand is moved slowly from the object towards the eyes. If the subject is sensitive, the eyes will usually close with a vibratory motion. In some cases the subject is then unable to open them, and the usual mesmeric phenomena can be obtained. If when the operator brings his hand near the eyes of the subject, the subject instead of closing them follows the movements of the fingers, the whole proceeding is repeated, but the subject is told to close his eyes when the fingers are brought near them, but to keep them fixed in the same direction as before, and to continue to think of the object and that only. The operator then for some minutes makes "passes," bringing his warm hands over and close to the face of the subject in one direction. When the subject is inclined to pass into the cataleptic state, an indication of his condition may be obtained by gently raising his arm; if he is beginning to be mesmerised, the arm remains in the position in which it is placed. If the arm falls, the mesmeric state may not infrequently be hastened on by telling the subject to keep his arm extended whilst he is still gazing at the object, or whilst the passes are being made. And that is the whole of the process. The man thus mesmerised sinks from manhood to a highly complicated piece of machinery. He is a machine which for a time is conscious, and in which ideas can be excited by appropriate stimulation; anyone acquainted with the machinery can set it in action.

The distinguishing feature of the earlier stages of mesmerism in man is that by slight stimulation any one centre can be easily set in violent activity, and its activity easily stopped, without the activity spreading to other distant centres. It is on this that the mesmeric phenomena usually exhibited depend; with most of these phenomena you are no doubt familiar, so that I need mention one or two only.

Complicated reflexes may be produced in various ways, just as we have seen is the case with a frog even when without its cerebral hemispheres. Thus Braid mentions that on one occasion an old lady who had never danced, and who indeed considered it a sinful pastime, when mesmerised began to dance as soon as a waltz tune was played.

A statement made to a subject will often produce implicit belief notwithstanding the evidence of his senses. I remember telling a subject that I was about to bring a hot body near his face, and he was to tell me when it was painful. I put my finger on his cheek, upon which he cried out violently that I was burning him. When he was awakened he remembered that I had touched him with something very hot. The idea I had given him was remembered, the evidence of his sense of touch was disregarded. Even in ordinary, apparently wakeful, life an idea may produce a belief in no way borne out by the evidence of the senses. Dr. Beard narrates that once when crossing the Atlantic, the steamer he was in ran into a sailing-vessel. It was at night, and amidst the roar of the wind, the shrieks, and cries, and prayers of the passengers, the cry went forth that the steamer was stove in and the bow sinking; straightway all eyes were turned to the bow, and to every eye it seemed to be sinking. "I shall never forget," he says, "how it gradually lowered in the darkness." In fact, however, the vessel was uninjured, and the bow did not sink at all. Here probably the majority of the people present passed simultaneously into a condition resembling the mesmeric condition; the idea presented to them by the cry "the bow is sinking" being more powerful than the ideas aroused by the objects actually seen.

But even in the absence of strong emotion, it may happen that an idea suggested by a statement may be more powerful than the proper sensory impression. There are some persons, apparently perfectly trustworthy, who nevertheless, if they were told to look closely at the top of this bell jar and see the faint flame coming from it, would very soon see the flame quite distinctly. In health, as well as in disease, there are many partial revelations of the condition produced by mesmerism.

In some subjects the sensibility of the skin to variations of temperature is very greatly increased, so that the contour and size of an object which is brought near the skin can be recognised by the alteration in the feeling of temperature of the part. The size and contour of an object such as a book or a coin being thus known, the subject may of course be able to guess that a book or coin is being held before him.

There are certain attitudes which we usually assume under the influence of certain moods or ideas; from each of the muscles concerned in bringing about any one attitude, impulses travel up to the brain, and give rise to a definite muscular sensation which comes therefore to be associated with a particular mental mood. In mesmerised people the production of a definite muscular sensation not infrequently produces in the mind the mood with which it is, in the wakeful state, associated. At the same time ideas may be produced corresponding to the mood, and the ideas may give rise to particular actions, such as laughing, crying, fighting.

If the head is pushed back and the shoulders opened out, the face assumes a look full of pride or haughtiness, and if the subject be asked what he is thinking about, he will give some answer indicating what a fine fellow he fancies himself to be. If, then, the head is bowed and the shoulders contracted, the aspect of the face changes to one of humility and pity. Occasionally it happens that a slight pressure on a single muscle, which causes it to contract, will by an irradiation of nerve impulses produce the muscular sensations proper to a group of muscles, and this will give rise to the associated frame of mind. Thus very different feelings may be made to rapidly succeed one another in the mind of the subject by simply pressing on various muscles of the head and neck. At first sight such an experiment looks like a revival of the now happily forgotten phrenology.

I have said that in a frog which remains mesmerised for any time, there is a considerable reflex depression, i. e. inhibition of the whole of the central nervous system—that there is an irradiation of inhibitory impulses. In man a similar irradiation of inhibitory impulses appears to take place; usually a mesmerised person if left alone passes gradually, but often rapidly, into a state of torpor; consciousness disappears, memory is lost, reflex action becomes difficult to obtain, finally, it may be, there is complete anæsthesia, a limb may be cut off without producing any movement or any pain; since this torpor comes on without anything farther being done to the subject, we may conclude that here, as in the frog, but to a much more marked degree, there is an irradiation of inhibitory impulses. The primarily inhibited centres send out inhibitory impulses to all other nerve centres. Up to a certain stage, possibly throughout, any one or more centres may be brought back to a condition of activity by certain exciting stimuli, but when these cease the inexcitable condition is soon brought back by the inhibitory impulses streaming to them from other nerve centres.

The extent to which the torpid condition develops itself, varies in different individuals. It depends upon the condition of the nervous system, upon the relative intensities of the inhibitory and exciting impulses. As far as our present knowledge goes, it would appear that a few only of those who can be mesmerised, can be made to pass into a condition of complete anæsthesia. It is possible, however, that this may be due to the passes which give rise to inhibitory impulses not being continued long enough. Dr. Esdaile, who in India was accustomed to mesmerise his patients before performing surgical operations upon them, used to continue the passes for one to two hours, and often to repeat this for several days in succession.

In different people the order in which different centres are inhibited varies, as we should expect from the unequal development of different centres in different people. This is no doubt of influence in determining whether the general state is cataleptic, somnambulistic, or lethargic, and here probably the method used to mesmerise is also of considerable importance; it would seem that the cataleptic condition is more likely to be developed when the process of mesmerisation involves a strain on the eyes of the subject, than when he is mesmerised by passes. Not much attention, however, has as yet been directed to this point.

There can, I think, be no doubt that mesmerism may help, and sometimes cure, persons suffering from certain diseases of the nervous system. It is not in our power to make any accurate statement of the way in which this is brought about; but since disease may be the result of either an over-activity or of an under-activity of any part of the central nervous system, it is reasonable to suppose that a beneficial effect will follow the employment of a method which allows us to diminish or increase these activities as we will. This is a side of the question which is of the greatest interest both to physicians and to physiologists—to physiologists, since it bears directly upon the problem of the influence of the nervous system on nutrition. There is good reason to believe that by directing attention strongly to any particular part of the body, the nutritive state of that part of the body may be altered. The determination of the actual way in which this is brought about is full of difficulties, but the following way is at least theoretically possible. It may be that the nerve centres connected with the tissue in question are made unusually active, and that they send out nerve impulses of a trophic nature, that is, impulses which directly control the nutrition of the tissue. The alteration in the tissue caused by its changed nutritive state—its changed metabolism—may conceivably be either beneficial or detrimental to the whole organism; it may give rise to a diseased state, or get rid of an existing one.

The modern miracles of healing, wrought in persons in a state of religious enthusiasm, offer a field for investigating this problem; the field, however, is a particularly bad one, and chiefly because so many people concerned regard any careful examination of the subject as impious. But in mesmerised persons it seems probable that such investigations could be made on a fairly satisfactory basis. Men when mesmerised gradually lose remembrance of those things which they remember when they are awake, but not infrequently other things are remembered which are forgotton in the waking state.* This is normally the case with a person who has been previously and recently mesmerised. He may then remember little else than what took place in the corresponding stage of his previous mesmerisation. In a certain state, then, an event or a command will produce in the central nervous system those changes which are necessary for the event or the command to be remembered later, without ever rising to consciousness in the waking condition. Thus a command to do a particular thing, given to a subject in this mesmeric stage, may be carried out when he awakes, although he is quite unconscious why he does it. We may say that such an act is one of unconscious memory. But it is, I think, something more than this. The subject is usually uneasy and preoccupied until the thing is done; he is to a greater or less extent unable to fix his attention on other things; he is, in fact, in a state of unconscious attention to an unconscious memory. This brings us to our point. It suggests that if a subject in a certain stage of mesmerisation be told that in a few days a sore will appear upon his hand, or conversely that a sore already there will disappear, the conditions which accompany conscious expectation and attention, will to a certain degree be established; and the trophic influence of the nervous system on the tissues may be tested in a manner which puts the experiment fairly within the control of the observer, and to a certain degree excludes imposture. Such an experiment has obviously some drawbacks, it would probably only succeed, if it succeeded at all, with a person whose nervous system was in a state of unstable equilibrium; and it can hardly be expected that the effects would be so striking as when conscious expectation is also concerned. Still observations of this kind are well worth attention, on account of the medical, the physiological, and the psychological issues involved in the results.

A lightly mesmerised subject can be easily brought back to a normal condition by a sudden slight shock, by sprinkling water in the face, or by a current of cold air. These give rise to exciting impulses which arouse to normal activity the inhibited parts of the brain; just as we have seen that any other part of the central nervous system can be aroused to activity by slight exciting impulses. There is no mystery in this, beyond the mystery which lies in the relative action of all exciting and inhibitory impulses. The power of responding in strikingly different ways to weak stimuli differing in kind, or to stimuli apparently of the same kind, but differing in intensity, is not peculiar to the nervous system of man ; it is a power possessed by the nervous system of all animals, and indeed, not improbably by all living substance. This has already been touched upon in what I have said of inhibition, but I will give you one or two other instances of the dissimilar effects produced by slight, and apparently not very dissimilar, stimuli, instances which are especially pertinent to the subject of mesmerism. These we owe to Heidenhain.

---

* A case is recorded by Braid, of a woman who, during natural somnambulism—which is almost identical with a state that can be produced by mesmerism—could repeat correctly long passages from the Hebrew Bible, and from books in other languages, although she had never studied any of these languages, and was quite ignorant of them when she was awake. At length, however, it was discovered that she had learnt the passages when she was a girl, by hearing a clergyman with whom she lived read them out aloud.

When morphia is given to a dog, and the animal is left undisturbed, it passes into a condition resembling sleep; but a little investigation usually shows that the condition differs in certain notable respects from sleep. Whilst consciousness, as far as can be told, is gone, and voluntary movement is abolished, many reflex actions can be obtained much more readily than in the waking state; moreover, there is a tendency for the muscles which contract in a reflex action to remain contracted, the nerve centres when set in activity remain active for a considerable time, and continue to send out impulses to the muscles, which in consequence are kept contracted; in other words the reflex contraction produced by a slight stimulus applied to the skin is of a tonic instead of a tetanic nature. Now this tonic contraction can be brought to an end by various slight stimuli, for instance by lightly stroking the skin over the contracted muscles, by gently tapping the contracted part, by blowing in the face of the animal, or by stimulating the cortex of the brain by a weak electric current. Nevertheless, the acts just mentioned may, when the muscles are not contracted, cause or help to cause, their contraction. I will give an instance of this. Electrical stimulation of a definite part of the cortex of the brain causes a tonic contraction of certain muscles of the leg, in consequence of which, let us say, the leg is bent and remains so. Now we have seen that passing the hand over the skin of the leg will cause it to unbend; well, if the cortex of the brain be stimulated with an electric current, not quite strong enough to produce of itself bending of the leg, the bending may at once be produced by gently stroking the leg at the same time as the cortex is being stimulated. Of a similar nature is the effect of electrical currents of different strengths. When a limb has been brought into a state of tonic contraction by electrical stimulation of a certain part of the cortex of the brain, a weaker electrical stimulation of the same spot of the cortex will bring the tonic contraction to an end.

The phenomena just described as occurring in a dog under the influence of morphia, closely resemble those often observed in human beings when mesmerised. Commonly in a mesmerised person the arm, let us say, may be made to bend by gently stroking the skin over the appropriate muscles; give a slight tap on the arm, and it relaxes. Braid observed in some subjects that if a limb was made rigid by passing the hands over it, and if it was left extended for a short time, then the repetition of the same act of passing the hands over the limb caused the rigidity to disappear. It is unnecessary, I think, to consider in detail the corresponding states in the narcotised dog and the mesmerised man; enough has been said to show that in both certain slight stimuli produce, sometimes excitation, sometimes inhibition.

It must, however, be noted, that our conception of inhibition is not rendered clearer by these facts; for it would appear from them that a nerve centre may be excited or be inhibited by the same nerve impulse, the result depending upon the condition of the nerve centre. This is not a necessary inference, but it is perhaps at present the most convenient working hypothesis. A certain group of facts, indeed, may be held together and receive a provisional explanation by saying that in some conditions of the central nervous system, a stimulus excites a nerve centre if it is quiescent, and inhibits it if it is active.

It seems to me probable that what is called the "transference of contracture" and the "transference of sensation" are of the same order of facts. These phenomena are exceedingly curious. Suppose that the left biceps of a mesmerised person be gently stroked or pressed, so that it contracts and remains so. The continuous contraction of the muscle is called contracture. In consequence of the contracture, the arm is kept bent. Suppose that the biceps of the other arm be gently stimulated, we may get a transference of the contracture, i. e. the right biceps becomes contracted and the right arm bent, whilst the left arm which previously was bent, falls flaccid. Similarly there may be a transference of sensation; thus the right arm say is rendered insensitive, so that pricking it with a needle does not give rise to any sensation; on the back of the right hand, a piece of metal, such as a two-shilling piece, is now placed, and left for a short time. On removing it, it is found that the spot of skin which was in contact with the metal has become sensitive, so that the prick of a needle is at once felt, but that the corresponding part of the other hand has become insensitive, so that pricking it with a needle produces no sensation.

The observations of this kind have hitherto been made almost, though not quite exclusively, upon patients suffering from certain diseases of the nervous system, and the facts have been described as occurring both in the wakeful and in the mesmeric state. The proximate explanation appears to be, to take the case of transference of sensation just mentioned, that the gentle tactile stimuli caused by the pressure of the coin on the skin, reaching an inhibited centre sets it in activity, and the sensibility of that part of the skin is restored, but the stimulus passes on to the corresponding and hitherto active centre of the opposite side of the body, and this is inhibited.

Here I must leave the subject. I have not attempted to give an account of all the phenomena of mesmerism; I have taken those phenomena which seemed to me to be the least easy to understand, the most liable to misconception, and have attempted to show that they resemble fundamentally certain simpler phenomena which can be observed in lower animals. I have further attempted to string together the various facts upon a thread of theory, which may be briefly summed up as follows:—

The primary condition of mesmerism is an inhibition of a particular mode of activity of the cortex of the brain, in consequence of which the will can no longer be made effective.

This inhibition may be brought about by nervous impulses coming from certain sensory nerves, as those of sight, touch, hearing.

It may also be brought about by impulses or changes arising in the cortex itself.

The inhibited cortex, and probably also inhibited lower centres of the brain, send out inhibitory impulses to all other parts of the central nervous system, so that the mesmerised man or animal gradually passes into a state of torpor, or even of complete anæsthesia.

The phenomena of the excitable stage of mesmerism are proximately determined by the possibility of exciting any particular centre alone, without exciting at the same time other centres by which its activity is normally controlled. In lower animals this stage is less marked in consequence of a greater interdependence of the various parts of the central nervous system.

I would expressly state that I regard this theory only as provisional. Further, I am quite conscious that it is very imperfect. A complete explanation of the phenomena of mesmerism and of its allied states can only be given when we have a complete knowledge of the structure and functions of all parts of the central nervous system. But I have not much doubt that the explanation of the main features of mesmerism will be found when we are able to answer the question—What is inhibition? And it is some comfort to think that the answer awaits us in the comparatively simple nervous system of the lower animals. I would not be understood to mean that variation of blood supply and various other events are of no influence in producing mesmeric phenomena; I think, however, that these events are of secondary importance only.

Finally, I would say a word about the attitude of physiologists to animal magnetisers and mesmerists. It has sometimes been made a subject of reproach to physiologists that they have not concerned themselves more actively in investigating mesmeric phenomena. The reproach has very little foundation. The knowledge which has been gained on the subject has been gained almost entirely by medical practitioners and by physiologists, and it must be remembered that until lately most physiologists were also medical practitioners; the division of labour is of recent date.

It is, however, true that in the beginning and middle part of this century there were many scientific men who regarded the subject with a contempt which intrinsically it did not deserve. But in my opinion they had much justification. A scientific man has always before him some problems which he knows he can solve, or help to solve. He has always before him a road which he knows leads somewhither. Mesmerism was long mixed up with assertions of the transmission of cerebral fluid, with impossible notions which had been banished from physiology, and with charlatanism. The scientific man of that day may, I think, be readily pardoned for supposing that the facts which were given as not more true than the theories, might be equally false. Why should he leave the fruitful work his hand had found to do for that which to all appearance would be barren.

Dr. Esdaile, who although himself not altogether free from blame for mystifying the subject, yet did much to advance it, expresses what must have been a general feeling:—"The ignorance and presumption of man; his passion for the mysterious and marvellous; his powers of self-delusion, with the pranks of knaves and the simplicity of fools, have so mystified the subject, that the artificial difficulties cost us more trouble to remove than the natural; and a mass of rubbish must be got rid of before we can reach the foundation stone of truth."

[J. N. L.]

WEEKLY EVENING MEETING,

Friday, March 21, 1884.

Sir William Bowman, Bart. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Matthew Arnold, Esq.

Emerson.

[For Abstract see Macmillan's Magazine for May, 1883.]

WEEKLY EVENING MEETING,

Friday, March 28, 1884.

SIR FREDERICK POLLOCK, Bart. M.A. Vice-President, in the Chair.

PROFESSOR OSBORNE REYNOLDS, M.A. F.R.S.

The Two Manners of Motion of Water.

IN commencing this discourse the author said :—

It has long been a matter of very general regret with those who are interested in natural philosophy, that in spite of the most strenuous efforts of the ablest mathematicians the theory of fluid motion fits very ill with the actual behaviour of fluids; and this for unexplained reasons. The theory itself appears to be very tolerably complete and affords the means of calculating the results to be expected in almost every case of fluid motion, but while in many cases the theoretical results agree with those actually obtained, in other cases they are altogether different.

If we take a small body such as a raindrop moving through the air, the theory gives us the true law of resistance; but if we take a large body such as a ship moving through the water, the theoretical law of resistance is altogether out. And what is the most unsatisfactory part of the matter is that the theory affords no clue to the reason why it should apply to the one class more than the other.

When, seven years ago, I had the honour of lecturing in this room on the then novel subject of vortex motion, I ventured to insist that the reason why such ill success had attended our theoretical efforts was because, owing to the uniform clearness or opacity of water and air, we can see nothing of the internal motion; and while exhibiting the phenomena of vortex rings in water rendered strikingly apparent by partially colouring the water, but otherwise as strikingly invisible, I ventured to predict that the more general application of this method, which I may call the method of colour-bands, would reveal clues to those mysteries of fluid motion which had baffled philosophy.

To-night I venture to claim what is at all events a partial verification of that prediction. The fact that we can see as far into fluids as into solids naturally raises the question why the same success should not have been obtained in the case of the theory of fluids as in that of solids? The answer is plain enough. As a rule, there is no internal motion in solid bodies; and hence our theory based on the assumption of relative internal rest applies to all cases. It is not, however, impossible that an, at all events seemingly, solid body should have internal motion, and a simple experiment will show that if a class of such bodies existed they would apparently have disobeyed the laws of motion.

These two wooden cubes are apparently just alike, each has a string tied to it. Now, if a ball is suspended by a string you all know that it hangs vertically below the point of suspension or swings like a pendulum. You see this one does so. The other you see behaves quite differently, turning up sideways. The effect is very striking so long as you do not know the cause. There is a heavy revolving wheel inside which makes it behave like a top.

Now what I wish you to see is, that had such bodies been a work of nature so that we could not see what was going on—if, for instance, apples were of this nature while pears were what they are—the laws of motion would not have been discovered; if discovered for pears they would not have applied to apples, and so would hardly have been thought satisfactory.

Such is the case with fluids: here are two vessels of water which appear exactly similar—even more so than the solids, because you can see right through them—and there is nothing unreasonable in supposing that the same laws of motion would apply to both vessels. The application of the method of colour-bands, however, reveals a secret: the water of the one is at rest, while that in the other is in a high state of agitation.

I am speaking of the two manners of motion of water—not because there are only two motions possible; looked at by their general appearance the motions of water are infinite in number; but what it is my object to make clear to-night is that all the various phenomena of moving water may be divided into two broadly distinct classes, not according to what with uniform fluids are their apparent motions, but according to what are the internal motions of the fluids which are invisible with clear fluids, but which become visible with colour-bands.

The phenomena to be shown will, I hope, have some interest in themselves, but their intrinsic interest is as nothing compared to their philosophical interest. On this, however, I can but slightly touch.

I have already pointed out that the problems of fluid-motion may be divided into two classes: those in which the theoretical results agree with the experimental, and those in which they are altogether different. Now what makes the recognition of the two manners of internal motion of fluids so important, is that all those problems to which the theory fits belong to the one class of internal motions.

The point before us to-night is simple enough, and may be well expressed by analogy. Most of us have more or less familiarity with the motion of troops, and we can well understand that there exists a science of military tactics which treats of the best manœuvres and evolutions to meet particular circumstances.

Suppose this science proceeds on the assumption that the discipline of the troops is perfect, and hence takes no account of such moral effects as may be produced by the presence of an enemy.

Such a theory would stand in the same relation to the movements of troops as that of hydrodynamics does to the movements of water. For although only the disciplined motion is recognised in military tactics, troops have another manner of motion when anything disturbs their order. And this is precisely how it is with water: it will move in a perfectly direct disciplined manner under some circumstances, while under others it becomes a mass of eddies and cross streams which may be well likened to the motion of a whirling, struggling mob where each individual particle is obstructing the others.

Nor does the analogy end here: the circumstances which determine whether the motion of troops shall be a march or a scramble, are closely analogous to those which determine whether the motion of water shall be direct or sinuous.

In both cases there is a certain influence necessary for order: with troops it is discipline; with water it is viscosity or treacliness.

The better the discipline of the troops, or the more treacly the fluid, the less likely is steady motion to be disturbed under any circumstances. On the other hand, speed and size are in both cases influences conducive to unsteadiness. The larger the army, and the more rapid the evolutions, the greater the chance of disorder; so with fluid the larger the channel, and the greater the velocity, the more chance of eddies.

With troops some evolutions are much more difficult to effect with steadiness than others, and some evolutions which would be perfectly safe on parade, would be sheer madness in the presence of an enemy. So it is with water.

One of my chief objects in introducing this analogy of the troops is to emphasise the fact, that even while executing manœuvres in a steady manner there may be a fundamental difference in the condition of the fluid. This is easily realised in the case of troops. Difficult and easy manœuvres may be executed in equally steady manners if all goes well, but the conditions of the moving troops are essentially different. For while in the one case any slight disarrangement would be easily rectified, in the other it would inevitably lead to a scramble. The source of such a change in the manner of motion under such circumstances, may be ascribed either to the delicacy of the manœuvre, or to the upsetting disturbance, but as a matter of fact, both of these causes are necessary. In the case of extreme delicacy an indefinitely small disturbance, such as is always to be counted on, will effect the change.

Under these circumstances we may well describe the condition of the troops in the simple manœuvre as stable, while that in the delicate manœuvre is unstable, i. e. will break down on the smallest disarrangement. The small disarrangement is the immediate source of the break-down in the same sense as the sound of a voice is sometimes the cause of an avalanche; but if we regard such disarrangement as certain to occur, then the source of the disturbance is a condition of instability.

All this is exactly true for the motion of water. Supposing no disarrangement, the water would move in the manner indicated in theory just as, if there is no disturbance, an egg will stand on its end; but as there is always slight disturbance, it is only when the condition of steady motion is more or less stable that it can exist. In addition then to the theories either of military tactics or of hydrodynamics, it is necessary to know under what circumstances the manœuvres of which they treat are stable or unstable. And it is in definitely separating these conditions that the method of colour-bands has done good service which will remove the discredit in which the theory of hydrodynamics has been held.

In the first place, it has shown that the property of viscosity or treacliness, possessed more or less by all fluids, is the general influence conclusive to steadiness, while, on the other hand, space and velocity are the counter influence; and the effect of these influences is subject to one perfectly definite law, which is that a particular evolution becomes unstable for a definite value of the viscosity divided by the product of the velocity and space. This law explains a vast number of phenomena which have hitherto appeared paradoxical. One general conclusion is, that with sufficiently slow motion all manners of motion are stable.

The effect of viscosity is well shown by introducing a band of coloured water across a beaker filled with clear water at rest. Now the water is quite still, I turn the beaker round about its axis. The glass turns but not the water, except that which is close to the glass. The coloured water which is close to the glass is drawn out into what looks like a long smear, but it is not a smear, it is simply a colour-band extending from the point in which the colour touched the glass in a spiral manner inwards, showing that the viscosity was slowly communicating the motion of the glass to the water within. To prove this I have only to turn the beaker back, and the colour band assumes its radial position. Throughout this evolution the motion has been quite steady—quite according to the theory.

When water flows steadily it flows in streams. Water flowing along a pipe is such a stream bounded by the solid surface of the pipe, but if the water be flowing steadily we can imagine the water to be divided by ideal tubes into a fagot of indefinitely small streams, any of which may be coloured without altering its motion, just as one column of infantry may be distinguished from another by colour.

If there is internal motion, it is clear that we cannot consider the whole stream bounded by the pipe as a fagot of elementary streams, as the water is continually crossing the pipe from one side to the other, any more than we can distinguish the streaks of colour in a human stream in the corridor of a theatre.

Solid walls are not necessary to form a stream: the jet from a fire hose, the falls of Niagara, are streams bounded by a free surface.

A river is a stream half bounded by a solid surface.

Streams may be parallel, as in a pipe; converging, as in a conical mouth-piece; or when the motion is reversed, diverging. Moreover, the streams may be straight or curved.

All these circumstances have their influence on stability in a manner which is indicated in the accompanying diagram:—

Circumstances conducive to

Direct or Steady Motion.	*Sinuous or Unsteady Motion.*
1. Viscosity or fluid friction which continually destroys disturbances. (Treacle is steadier than water.)	5. Particular variation of velocity across the stream, as when a stream flows through still water.
2. A free surface.	6. Solid bounding walls.
3. Converging solid boundaries.	7. Diverging solid boundaries.
4. Curvature with the velocity greatest on the outside.	8. Curvature with the velocity greatest on the inside.

It has for a long time been noticed that a stream of fluid through fluid otherwise at rest is in an unstable condition. It is this instability which gives rise to the talking-flame and sensitive-jet with which you have been long familiar in this room. I have here a glass vessel of clear water in front of the lantern, so that any colour-bands will be projected on the screen.

You see the ends of two vertical tubes one above the other. Nothing is flowing through these tubes, and the water in the vessel is at rest. I now open two taps, so as to allow a steady stream of coloured water to enter at the lower pipe, water flowing out at the upper. The water enters quite steadily, forms a sort of vortex ring at the end which proceeds across the vessel, and passes out at the lower tube. Now the coloured stream extends straight across the vessel, and fills both pipes. You see no motion; it looks like a glass rod. The water is, however, flowing slowly along it. The motion is so slow, that the viscosity is paramount, and hence the stream is steady.

I increase the speed, you see a certain wriggling sinuous action in the column; faster, the column breaks up into beautiful and well-defined eddies, and spreads out into the surrounding water, which, becoming opaque with colour, gradually draws a veil over the experiment.

The same is true of all streams bounded by standing water. If the motion is sufficiently slow, according to the size of the stream and the viscosity of the fluid, it is steady and stable. At a certain critical velocity, the which is determined by the ratio of the viscosity to the diameter of the stream, the stream becomes unstable. Under any conditions, then, which involve a stream flowing through surrounding water, the motion will be unstable if the velocity is sufficient.

Now, *one* of the most marked facts relating to experimental hydrodynamics is the difference in the way in which water flows along contracting and expanding channels; these include an enormously large class of the motions of water, but the typical phenomenon is shown by the simple conical tubes. Such a tube is now projected on the screen; it is surrounded with clear still water. The mouth of the tube at which the water enters is the largest part, and it contracts uniformly for some way down the channel, then the tube expands again gradually until it is nearly as large as at the mouth, and then again contracts to the tube necessary to discharge the water. I draw water through the tube, but you see nothing as to what is going on. I now colour one of the elementary streams outside the mouth; this colour-band is drawn in with the surrounding water, and will show us what is going on. It enters quite steadily, preserving its clear streak-like character until it has reached the neck where convergence ceases; now the moment it enters the expanding tube it is altogether broken up into eddies. Thus the motion is direct in the contracting tube, sinuous in the expanding.

The hydrodynamical theory affords no clue to the cause why; and even by the method of colour-bands the reason for the sinuosity is not at once obvious. If we start the current suddenly, the motion is at first the same in both tubes, its change in the expanding pipe seemed to imply that here the motion was unstable. If so, this ought to appear from the equations of motion. With this view this case was studied, I am ashamed to say how long, without any light. I then had recourse to the colour-bands again, to try and see how the phenomena came on. It all then became clear: there is an intermediate stage. When the tap is opened, the immediately ensuing motion is nearly the same in both parts; but while that in the contracting portion maintains its character, that in the expanding portion changes its character. A vortex ring is formed which, moving forward, leaves the motion behind that of a parallel stream through the surrounding water.

If the motion be sufficiently slow, as it is now, this stream is stable, as already explained. We thus have steady or direct motion in both the contracting and expanding parts of the tube, but the two motions are not similar: the first being one of a fagot of similar elementary contracting streams, the latter being that of one parallel stream through the surrounding fluid. The first of these is a stable form; the second an unstable form, and, on increasing the velocity, the first remains, while the second breaks down; and we have, as before, the expanding part filled with eddies.

This experiment is typical of a large class of motions. Wherever fluid flows through a narrow, as it approaches the neck it is steady, after passing, it is sinuous. The same effect is produced by an obstacle in the middle of a stream; and very nearly the same thing by the motion of a solid object through the water.

You see projected on the screen an object not unlike a ship. Here the ship is fixed, and the water flowing past it; but the effect would be the same if we had the ship moving through the water. In the front of the ship the stream is steady, and so till it has passed the middle, then you see the eddies formed behind the ship. It is these eddies which account for the discrepancy between the actual and theoretical resistance of ships. We see, then, that the motion in the expanding channel is sinuous because the only steady motion is that of a stream through water. Numerous cases in which the motion is sinuous may be explained in the same way, but not all.

If we have a perfectly parallel channel, neither contracting nor expanding, the steady moving stream will be a fagot of perfectly steady parallel elementary streams all in motion, but moving fastest at the centre. Here we have no stream through steady water. Now when this investigation began it was not known, or imperfectly known, whether such a stream was stable or not, but there was a well-known anomaly in the resistance to motion in parallel channels. In rivers, and all pipes of sensible size, experience had shown that the resistance increased as the square of the velocity, whereas in very small pipes, such as represent the smaller veins in animals, Poiseuille had proved the resistance increased as the velocity.

Now since the resistance would be as the square of the velocity with sinuous motion, and as the velocity, if direct, it seemed that the discrepancy could be accounted for if the motion could be shown to become unstable for a sufficiently large velocity. This suggested the experiment I am now about to produce before you.

You see on the screen a pipe with its end open. It is surrounded by clear water and by opening a tap I can draw water through it. This makes no difference to the appearance until I colour one of the elementary streams, when you see a beautiful streak of colour extend all along the pipe. The stream has so far been running steadily, and appears quite stable. I now merely increase the speed; it is still steady, but the colour-band is drawn down fine. I increase the colour and then again increase the speed. Now you see the colour-band at first vibrates and then mixes so as to fill the tube. This is at a definite velocity; if the velocity be diminished ever so little the band becomes straight and clear; increase it again, it breaks up. This critical speed depends on the size of the tube in the exact inverse ratio; the smaller the tube, the greater the velocity; also, the more viscous the water the greater the velocity.

We have then not only a complete explanation of the difference in the laws of resistance generally experienced and that found by Poiseuille, but also we have complete evidence of the instability of parallel streams flowing between or over solid surfaces. The cause of the instability is as yet not explained, but this much can be shown, that whereas lateral stiffness in the walls is unimportant, inextensibility or tangential rigidity is essential to the creation of eddies. I cannot show you this because the only way in which we can produce the necessary conditions without a solid channel is by a wind blowing over water. When the wind blows over water it imparts motion to the surface of the water just as a moving solid surface; moving in this way, however, the water is not susceptible of eddies. It is unstable, but the result of disturbance is waves. This is proved by an experiment long known, but which has recently attracted considerable notice.

If oil be put on the surface it spreads out into an indefinitely thin sheet which possesses only one of the characteristics of a solid surface, it offers resistance, very slight, but still resistance to extension and contraction. This, however, is sufficient to entirely alter the character of the motion. It renders the water unstable internally, and instead of waves, what the wind does is to produce eddies beneath the surface. This has been proved, although I cannot show you the experiments.

To those who have observed the phenomena of oil preventing waves, there is probably nothing more striking throughout the region of mechanics. A film of oil so thin that we have no means of illustrating its thickness, and which cannot be perceived except by its effect—which possesses no mechanical properties that can be made apparent to our senses—is yet able to entirely prevent an action which involves forces the strongest we can conceive, which upset our ships and destroy our coasts. This, however, becomes intelligible when we perceive that the action of the oil is not to calm the sea by sheer force, but merely, as by its moral force, to alter the manner of motion produced by the action of the wind from that of the terrible waves upon the surface into the harmless eddies below. The wind throws the water into a highly unstable condition, into what morally we should call a condition of great excitement. The oil by an influence we cannot perceive directs this excitement.

This influence, though insensibly small, is however now proved of a mechanical kind, and to me it seems that the phenomenon of one of the most powerful mechanical actions of which the forces of nature are capable, being entirely controlled by a mechanical force so slight as to be otherwise quite imperceptible, does away with every argument against the strictly mechanical sources of what we may call mental and moral forces.

But to return to the instability in parallel channels. This has been the most complete, as well as the most definite result of the colour-bands.

The circumstances are such as to render definite experiments possible. These have been made, and reveal a definite law of the instability, which law has been tested by reference to all the numerous and important experiments on the resistance in channels by previous observers; whereupon it is found that waters behave in exactly the same manner whether the channel, as in Poiseuille's experiment, is of the dimensions of a hair or whether it be the size of a water main or of the Mississippi; the only difference being that in order that the motions may be compared, the velocity must be inversely as the diameter of the pipe. But this is not the only point explained if we consider other fluids than water. Some fluids, like oil or treacle, apparently flow more slowly and steadily than water. This, however, is only in smaller channels; the critical velocity increases with the viscosity of the fluid. Thus, while water in comparatively large streams is always above its critical velocity, and the motion always sinuous, the motion of treacle in streams of such size as we see is below its critical velocity, and the motion direct. But if nature had produced rivers of treacle the size of the Thames, for instance, the treacle would have flowed just like water. Thus, in the lava streams from a volcano, although looked at close the lava has the consistence of a pudding, in the large and rapid streams down the mountain sides the lava flows as freely as water.

I have now only one circumstance left to which to ask your attention. This is the effect of curvature of the stream on the stability of the fluid.

Here again we see the whole effect altered by very slight causes.

If water be flowing in a bent channel in steady streams, the question as to whether it will be stable or not turns on the variation in the velocity from the inside to the outside of the stream.

In front of the lantern is a cylinder with glass ends, so that the light passes through in the direction of the axis. The disk of light on the screen being the light which passes through this water, and is bounded by the circular walls of the cylinder.

By means of two tubes temporarily attached, a stream of coloured water is introduced right across the cylinder extending from wall to wall; the motion is very slow, and the taps being closed, and the tubes removed, the colour-band is practically stationary. The vessel is now caused to revolve about its axis. At first, only the walls of the cylinder move, but the colour-band shows that the water gradually takes up the motion, the streak being wound off at the ends into a spiral thread, but otherwise remaining still and vertical. When the spirals meet in the middle, the whole water is in motion, but the motion is greatest at the outside, and is therefore stable. The vessel stops, and gradually stops the water, beginning at the outside. If the motion remained steady, the spirals would unwind, and the streak be restored. But the motion being slowest at the outside against the surface, you will see eddies form, breaking up the spirals for a certain distance towards the middle, but leaving the middle revolving steadily.

Besides indicating the effect of curvature, this experiment really illustrates the action of the surface of the earth on the air moving over it; the varying temperature having much the same influence as the curvature of the vessel on stability. The air is unstable for a few thousand feet above the surface, and the motion is sinuous, resulting in the mixing of the strata, and producing the heavy cumulus clouds; but above this the influence of temperature predominates, and clouds, if there are any, are of the stratus-form, like the inner spirals of colour. But it was not the intention of this lecture to trace the two manners of motion of fluids in the phenomena of Nature and Art, so I thank you for your attention. [O. R.]

WEEKLY EVENING MEETING,

Friday, April 4, 1884.

Sir Frederick Bramwell, F.R.S. Manager and Vice-President, in the Chair.

Professor T. G. Bonney, D.Sc. F.R.S. Pres. G.S.

The Building of the Alps.

When were the Alps upraised, and what is the age of their building stones? On the former of these questions there is less diversity of opinion than on the latter; yet, notwithstanding all that has been written on both, I am not without hope that I may find a few things sufficiently novel to be of interest to a general audience.

The subject, indeed, is so vast that I must crave your indulgence for leaving some gaps in my reasoning unfilled, and presenting you with little more than an outline. To save time I shall assume a knowledge of the simpler geological terms, asking you only to remember that I always use the word "schist," as I maintain it ought to be used, to denote a more or less fissile rock the constituents of which have undergone so much mineral change that, as a rule, their original nature is almost wholly a matter of conjecture. I must also ask you to remember that, though I have seldom mentioned the names of other workers, I am really doing little more than giving an epitome of the labours of a host of geologists, conspicuous among whom are Heim, Baltzer, Von Hauer, Gastaldi, Lory, Favre, Renevier, and many more, both Continental and English; I select however those facts with which I have myself become familiar during many visits to different districts of the Alps, from the Viso on the south to the Dachstein on the east.

It is needless, I assume, to explain that mountain chains are the result of lateral thrust rather than of vertical upheaval, and their contours are mainly due to the sculpturing action of heat and frost, rain and rivers, acting upon rocks bent into various positions, and of various degrees of destructibility. There are, however, three principles which are less familiar, but which I must ask you to bear in mind throughout this lecture: (1) That when a true schist is asserted to be the metamorphosed representative of a post-Archæan rock, the *onus probandi* lies with him who makes the assertion; (2) that rocks composed of the detritus of older rocks may often readily be mistaken for them; (3) that great caution is needful in applying the principles of lowland stratigraphy to a mountain region. The first of these is, I know, disputed, but there can be little doubt as to its accuracy, the second is indisputable; so is the third; but I will briefly illustrate what I mean by the statement.

[Attention was then directed to diagrams of folds and reversals of strata in the Alps.]

The first section to which I invite your attention is in the neighbourhood of the Lake of Lucerne. There are few travellers to whom the cliffs of the Rigi are not familiar. Those great walls of rock, along and beneath which the Rigibahn now takes its audacious way, are mainly composed of enormous masses of conglomerate, an indurated gravel of Miocene age, called the nagelflue. These pebble beds may be traced in greater or less development along the north-western margin of the Swiss Alps; they attain in the Rigi and the fatal crags of the adjoining Rossberg a thickness of not less than 2000 feet. The structure and nature of this nagelflue show that it has been deposited by rivers, possibly at their entry into lakes, but more probably, as suggested by my friend Mr. Blanford, on beginning a lowland course at the very gates of the mountains. In this great mass there are indeed pebbles of doubtful derivation; but we need not hesitate to refer the bulk of them to the mountains which lie towards the east, and we may regard the great pebble beds of the Rigi and the Rossberg as built of the ruins of Miocene Alps by the streams of a Miocene Reuss. Now, when we scrutinise the pebbles of this nagelflue we are at once struck by a remarkable fact. The Reuss, at the present day, only passes through mesozoic rocks when it approaches the neighbourhood of the Lake of Lucerne. It is within the mark to say that quite three-fourths of its drainage area consists of crystalline rocks. Hence schists and gneisses abound among its pebbles, and the same rocks are no less frequent among the erratics which have been deposited by the vanished glaciers of the Great Ice Age on the flanks of the Rigi to a height of 2000 feet above the Lake of Lucerne. Yet, on examining the nagelflue, we find that, while pebbles of grit, and limestone, and chert—specimens of the Alpine mesozoic rocks—abound, pebbles of schist and gneiss are extremely rare. I had searched for hours before I found a single one. The matrix also of the nagelflue—the mortar which makes this natural concrete—when examined beneath the microscope, tells the same story. We do not see in it the frequent quartz grains, the occasional pieces of felspar, the mica flakes, which are records of the detrition of gneissic rocks, but it consists of fragments similar to those which form the larger pebbles. It is therefore a legitimate inference that, in this part of the Alps at least, the protective covering of mesozoic rock in the Miocene age had not generally been stripped away from the crystalline schists of the Upper Reuss, and that since then the mountains may have been diminished and the valleys deepened by at least a mile vertically. I have spoken only of the valley of the Reuss, but a little consideration will show that my remarks may be extended to a much larger area of the Oberland Alps.

I pass now to two other sections: of these the first is in the neighbourhood of Pontresina. Most of the peaks in this region consist of igneous rocks, of gneisses, and of schists, but some of later date are not wanting—as, for example, may be seen in the flanks of well-known Heuthal. These last are limestones of Triassic age. Here they overlie unconformably a coarse gneiss—in other places they rest on schists presumably of later date; in fact, the series of mesozoic rocks of which the above limestone is the lowest member—though now to a great extent removed by denudation—has clearly once passed transgressively over the whole series of gneisses and schists of the Engadine.

The second section, or rather group of sections, is some distance away to the south-east, in the region of the Italian Tyrol. Those magnificent crags of the Dolomite mountains, the serrate teeth of the Rosengarten and the Langkofel, the towers of the Cristallo and the Drei Zinnen, the precipitous masses of the Blattkogel and the Marmolata, are built up of rocks of Triassic age, not of a very different date from the soft red marls which occupy so large an area in the Midlands of England. Follow me for one moment by the mountain road from Predazzo to Primiero. At the former place—classic ground for geologists—we are surrounded by great masses of igneous rocks, the roots, it may be, of long-vanished cones, although we refuse to recognise a crater in the valley about Predazzo. As we ascend towards the beautiful Alps of Paneveggio, we pass for a considerable distance over a great mass of red felstone. This belongs to a group of igneous rocks which extend to the westward even beyond the Etsch. It is overlain by the beds of the Trias, commencing with the red Grodner sandstone and passing up soon into the vast masses of Dolomite which form the wild crags of the Cimon della Pala and its attendant summits. But as we descend on the other side of the pass towards Primiero we see the Triassic rocks, without the intervention of the felstone, resting upon mica schists, similar to those which occur in many other parts of the Alps. Sections of the above kind, were it needful, might be multiplied indefinitely to prove that between the base of the Trias and the Alpine schists and gneisses there is an enormous break, but we may content ourselves with one other, interesting not only for the completeness of the demonstration but also for the mode in which it illustrates Alpine structure.

[Attention was then directed to the section of the Mont Blanc Range, after Favre.]

The Aiguilles Rouges are composed of coarse gneisses and crystalline schists, but on the highest summit there remains a fragmental outlier of stratified and unaltered rock. The upper part of this is certainly Jurassic. Below this comes a representative of the Trias—much attenuated, as it is generally in this western region, with possibly a remnant of a deposit of Carboniferous age. Be that as it may, there is undoubtedly here a great break between the crystalline series and the succeeding mesozoic or palæozoic rock.

There remains yet one other section to which I wish to direct your attention; it is near Vernayaz, in the vicinity of the famous gorge of the Trient. Where the Rhone bends, at Martigny, from a south-west to a north-west course, the crystalline *massif* of the Mont Blanc region of which we have just spoken crosses the river, and is lost to sight as it plunges beneath the mesozoic rocks of the western summits of the Oberland. The gorge of the Trient is cut through hard and moderately coarse gneiss; the same rock occurs at the Sallenche waterfall. Between the two is a mass of rock of a totally different character—in part a dark slate, like some in Britain of Lower Silurian age; in part a conglomerate or breccia in a micaceous matrix, proved by its plant remains to be a member of the Carboniferous series. Omitting some minor details, not without interest, it may suffice to say that we have in this place the end of an almost vertical loop, formed by the folding of beds of Carboniferous age between the crystalline rocks, which are the foundation-stones of the district. The conglomerate is at the base of the Carboniferous series, and its matrix so closely resembles a mica schist, that it has been claimed as indicating metamorphism, and as linking together the Carboniferous slates and the crystalline schists. But, in the first place, the fragments in the conglomerate are not only gneisses and schists, but also ordinary slaty rocks, no more altered than those of Llanberis. How, we may well ask, could the latter escape unchanged when all the surrounding matrix was converted into mica schist? Further, when we apply the test of the microscope—that Ithuriel spear by which the deceits of rocks are so often revealed—we find that this seeming mica schist is only the consolidated *débris* of micaceous rocks. Its composition, and that of the conglomerate, justifies us in asserting that when the Carboniferous rocks of the Valorsine were deposited there were land surfaces of gneiss and schist in the western region of the Alps, and that these rocks were substantially identical with those through which the Trient has sawn its ravine.

It would be easy to multiply instances similar to one or the other of those quoted above from this or that district of the Alpine region, from the south of Monte Viso to the north of the Adriatic, to speak only of those districts of which I have a personal knowledge; but I should speedily weary you, and will ask you to regard these as typical cases, single samples of a great collection. They justify, as I think you will agree, the following inferences: (1) That there has been one epoch, at least, of mountain-making posterior to the deposition of the Miocene nagelflue, which has given to many parts of the Alpine chain an uplift sometimes not less than a mile in vertical elevation; (2) that prior to this there was an earlier epoch of mountain-making, which affected all the rocks of older date, including at any rate a portion of middle Eocene age—for we find marine strata of this date crowning the summit of the Diablerets, now more than 10,000 feet above the sea, and bent back, as at the Rigi Scheideck, over the beds of the nagelflue; (3) that there was a pre-Triassic land surface of great extent, largely composed of crystalline rocks, and that with this geological age commenced a long continuous period of depression, lasting into Tertiary times; (4) that a land surface of considerable extent existed at a yet earlier period, and that this in the Carboniferous age was watered by streams and clothed with vegetation—whether there were mountains then it is impossible to say, but the evidence certainly points to the conclusion that the ground was hilly; (5) that anterior to the last-named period there is a great gap in our records; the older rocks, whose stratigraphical position can be ascertained, being much metamorphosed, so that we appear justified in concluding that all the more important mineral changes which they had undergone occurred in pre-Carboniferous times—namely, that the later Palæozoic land surfaces consisted of gneiss and schists in all important respects identical with those which now exist.

I have thus led you step by step—by processes, I trust, of cautious induction—to the result that the Alps, as an irregular land surface, are a very ancient feature in the contour of the earth, and that the gneisses and crystalline schists, whereof they so largely consist, are rocks of very great antiquity. Let us now attempt to advance a step further by attacking the problem from another side. Hitherto we have been working downwards from the newer to the older, from the rocks of known towards those of unknown date. Beginning now in the unknown, beginning with the most remote that we can find, let us proceed onwards toward the more recent and more recognisable.

This is a task of no slight difficulty. The ordinary rules of stratigraphical inference frequently fail us; nay, if blindly followed, would lead us to the most erroneous conclusions. In the apparent succession of strata in a mountain range the last may be first and the first last in the literal sense of the words. Beds may be repeated again and again by great folds, now in the direct, now in the inverse order of their superposition. They may have been faulted and then folded, or folded and then faulted, and the difficulty is augmented by the vast scale on which these earth movements have taken place, by the frequent impossibility of scaling the crags or pinnacles where critical sections are disclosed, and by the masking of large areas of surface by snow and glacier, or by *débris* and vegetation. Yet more, the consciousness of these difficulties produces in the mind—I speak for myself—a sort of hesitation and scepticism, which are most unfavourable for inductive reasoning. Knowing not what features are of importance, one is perplexed by the variety of facts that seem to call for notice; knowing how easily one may be deceived, one hesitates to draw conclusions. I am often painfully conscious of how much I have lost in a previous journey from not having remarked some fact to which a fortunate accident has just compelled my attention. In this part, therefore, I must be pardoned if I speak with considerable hesitation and do not attempt more than to state those inferences which seem to me warranted by facts.

I shall again ask permission to conduct you to a series of typical sections, which, however, I shall describe with less minuteness.

Let us place ourselves in imagination on the great ice-field at the upper part of the Gross Aletsch Glacier—the Place de la Concorde of Nature, as it has been happily termed. We are almost hemmed in by some of the loftiest peaks of the Bernese Oberland: the Aletschhorn, the Jungfrau, the Mönch, and several others. We find the rocks which rise immediately round the glacier—as, for example, near the well-known Concordia hut—to be coarse gneisses, with difficulty distinguishable from granites. As the eye travels up any one of the mountain ridges, the rock evidently becomes less massive and more distinctly foliated. We note the same sequence as we retrace our steps towards the Rhone valley—speaking in general terms, the ridges and the flanks of the Eggischhorn consist of more finely granulated gneisses and of strong micaceous schists, which alternate more frequently one with another. Further to the west, in the region around the Oberaletsch Glacier and on the slopes of the Bell Alp, we find the same succession—coarse granitoid gneisses in the relatively lower part of the heart of the chain, finer grained and more variable gneisses and schists on the upper ridges and the southern flanks.

Let us change our position to a spot considerably to the east, to the great section of the crystalline series made by the valley of the Reuss below Andermatt.

From the spot where the rocks close in suddenly upon the torrent near the Devil's Bridge, to a considerable distance below Wasen, extends an almost unbroken mass of coarse granitoid gneiss. This, however, becomes more distinctly bedded and schistose before it entirely disappears beneath the secondary deposits that border the Bay of Uri. Similarly, if from Wasen, where the gneiss is barely distinguishable from granite, we ascend the wild glen which leads up to the Susten Pass, and descend on the other side by the grand scenery of the Stein Alp to the beautiful Gadmenthal, thus passing obliquely outwards along the apparent strike of the rocks to the point where, as in the neighbourhood of Imhof, they finally disappear beneath mesozoic deposits, we again find that we are among rocks which are rather more variable in their mineral character, oscillating between moderately coarse gneisses, sometimes porphyritic, and strong mica schists. Near Mühlestalden, in the Gadmenthal, even a bed of white crystalline dolomitic limestone is interstratified with the gneissic rocks.

Leaving for a brief space the vicinity of the St. Gothard road, and returning to the upper valley of the Rhone, let us place ourselves on such an outlook as we can obtain from Professor Tyndall's châlet on the Bel Alp, and fix our eyes on the magnificent panorama of the Pennine chain, with whose geology we will suppose ourselves to have become familiar in frequent traverses from the northern to the southern side of the watershed of Central Europe. Facing us, and forming the lower slopes and crags of the great mountain chain of the Pennines, we see an enormous mass of distinctly bedded rock, of a brownish tint, of which at this distance we should hesitate to say whether we ought to regard it as a member of the metamorphic or of the ordinary sedimentary series. In an E.N.E. direction we see it gradually rising to form the peak of the Ofenhorn and the upper part of the mountains about the Gries Pass. In the opposite direction it forms the lower slopes of the Simplon Pass and the portals of the valley of the Visp. Hence, could we follow it, the area occupied by this rock broadens out into the spurs which enclose the Einfischthal and the Eringerthal, and crosses the watershed towards the south, to the east of the St. Bernard Pass. In more than one locality in the region of the Binnenthal a band, of no great vertical thickness, of a white crystalline dolomite is conspicuously present. A very similar group of rocks occurs in the Val Piora, in some bands of which black garnets are very abundant. The same mineral also occurs in a similar rock near the summit of the Gries Pass. Andalusite or staurolite also occurs occasionally; the group, in short, is well characterised, and for reference I will call it the Lustrous Schists.

I pass now to the neighbourhood of the St. Gothard. The coarse gneiss, which is pierced by the northern entrance of the great tunnel, ends abruptly at the Urnerloch. The basin of the Urserenthal is excavated from satiny slates with dark limestones, very possibly of Jurassic age, and from some underlying rather variable schists. The first rock visible on the eastern side as we approach Andermatt is a schistose crystalline limestone, associated with mica schists; and a series of rather variable schists, evidently very different from the coarse gneisses of the gorge below, appears to cross the valley, and form the slopes leading to the Oberalp Pass. These may be traced for some distance up the Furka road above Realp, when they are abruptly succeeded by the slaty group mentioned above. I am convinced that they are much more ancient than the latter, being probably members of the Lustrous Schist group, if not older. It is obvious that the newer rocks are only a fragment of a loop of a huge fold, over which on either hand the fragments of the enveloping older metamorphic rocks tower up in mountain peaks. On the ascent of the St. Gothard Pass from Hospenthal a series of somewhat variable micaceous schists continues till the top of the first step in the ascent is reached, about 800 feet above the valley, when gneiss sets in, generally rather coarse and sometimes very porphyritic, occasionally interbanded with dark, rather friable mica schists. The upper plateau of the pass consists of a porphyritic rock, often called granite, but with a gneissose aspect and rather more friable in character than the rock of the Wasen district. On the first steep descent on the south side this rock appears to pass into a normal coarse gneiss, occasionally banded with mica schist, resembling that in a similar position on the northern flank, which is succeeded for a short space by a remarkably well-banded gneiss. To this succeeds—it must be remembered that the series is inverted in order—the great group of hornblendic and garnetiferous mica schists, which continue along the Val Tremola and the lower slopes of the mountain to the neighbourhood of Airolo, where some calcareous rock occurs, being probably an infold of much later date.

Through the kindness of Mr. Fletcher and Mr. Davis, of the British Museum, I have been allowed to examine the series of specimens from the St. Gothard tunnel in that collection. They correspond in general with the succession above indicated, except that I have failed to identify the granitoid rock of the summit plateau. Leaving, however, for a moment the question of correlation, we see that the St. Gothard section presents us with an instance of folding on a gigantic scale, and of the fan structure, doubtless with many minor flexures and faults.

In the neighbourhood of the Val Piora we get an important succession. The ascent to the hotel from the Val Bedretto passes in the main over a series of micaceous schists, and rather friable gneisses, which are a prolongation of an axis exposed in the mountains south of Airolo and fairly correspond with much of the rock (excepting the granitoid) forming the upper part of the St. Gothard Pass. To this succeeds a series which, though more calcareous, clearly represents the garnetiferous actinolitic series of the southern slopes, and to this a group closely resembling the Lustrous Schists.

I pass now to the section of the Simplon. On the southern side, deep in the glen of the Doveria, in the vicinity of the gorge of Gondo, we find a mass of granitoid gneiss, which recalls to mind that already described from the wildest portion of the upper valley of the Reuss. We may, I think, with confidence affirm that, whatever be the true nature of this rock, we are again touching the foundation-stones of the rock masses of the Alps. As we approach Algaby, the granitoid gneiss becomes more distinctly bedded and variable, a thin band of micaceous crystalline limestone is passed, and presently the more rapid ascent of the pass begins. Hence to beyond the summit we traverse, so far as can be seen, a great series of bedded gneisses, often coarse and even porphyritic, and of schists. The same are displayed in the crags of Monte Leone on the east and of the Rossbodenhorn on the west. As shown in Professor Renevier's valuable section, bands of crystalline Dolomitic limestone, and of hornblendic and garnetiferous schists occur in various places on either side of the Simplon road. Then, after descending about half way to Brieg, we strike the group of the Lustrous Schists, with the usual calcareous zone in the lower part. Professor Renevier does not attempt to unravel the complexities of the strata which compose this portion of the central ridge of the Alps, and I feel that my slighter knowledge makes caution yet more imperative; but I think we are justified in asserting that we have evidence of an upward succession from the coarse granitoid fundamental gneisses, through more variable and bedded gneisses, to a group which recalls the garnetiferous schists, so finely developed on the southern flanks of the St. Gothard—a group also traceable in the upper portion of the Binnenthal, though apparently far less perfectly developed. I think also that in the gigantic anticlinal of the Simplon we have evidence of sharp flexures on a great scale; and that these garnetiferous schists are only here and there preserved as the lower ends of enfolded loops, so that the bulk of the *massif*, and so far as I can tell the actual summit ridges of the Rossbodenhörner and Monte Leone, are composed of the bedded gneisses and strong schists, and perhaps of the more friable gneisses which have been already described in the mountains further to the east.

The mountains further west—the aspiring peaks which rise around the two branches of the Visp, including among them some of the highest summits of the Alps, such as Monte Rosa, the Mischabelhörner, the Matterhorn, and the Weisshorn—offer indeed magnificent sections, but are full of difficulty. The fundamental gneiss, if I mistake not, is occasionally exposed—as, for example, in the rocks of Auf der Platte, at the base of Monte Rosa; and in parts of the Mischabelhörner blocks of coarse granitoid rock, often very porphyritic, which I refer to the same series, are brought down by the glaciers. There are also mica schists in plenty, such as the summit rocks of Monte Rosa and the backbone—if the phrase be permitted—of the Mischabel- and Saaser- hörner, which I refer to the second zone already described—that of the bedded gneisses and strong mica schists. I have also seen specimens which closely resemble the garnetiferous schists of the St. Gothard district, but we meet in this district with a group of rocks which, if not altogether unknown before, appears now to be developed to an exceptional extent, and to become an important factor in the Alpine crystalline series.

Those who are familiar with the environs of Saas and Zermatt will remember how frequently schists or schistose rocks of a greenish colour occur. Sometimes they are interbedded with strong mica schists, or schisty quartzites; sometimes they form homogeneous masses of considerable extent. It is possible that some of the latter are intrusive masses of serpentine, to which subsequent pressure has given a schistose aspect; certainly there are occasional masses of coarse gabbro, which I think undoubtedly an intrusive igneous rock; but still, making all allowance for such cases, there is in this region a considerable mass of greenish hornblendic, talcose, and serpentinous rocks which appears to be non-igneous in origin. We find these all around Zermatt. They form the ridges of the Gorner Grat and of the Hörnli. They break out through the snows of the Breithorn and Little Mont Cervin, and constitute no inconsiderable portion of the mighty obelisk of the Matterhorn. The whole of that peak, according to the investigations of Sgr. Giordano—and with this my own recollections correspond—consists of an apparently regularly bedded series of serpentinous and micaceous schists, and of greenish gneisses, with the exception of a gabbro, developed on the western side, which I have no doubt is an intrusive rock. Can we trust these indications? Are we justified in assigning to this zone, with those characteristics, a vertical thickness of more than a mile? To these questions I can give at present no answer, further than to state that I am convinced that, notwithstanding the apparent regularity of the bedding in this and the neighbouring peaks, there are really great folds which patient scrutiny may at length unravel, and that this zone of greenish rocks—for which Alpine geologists have proposed the name of *Pietra Verde* group, appears to underlie the garnetiferous series of silvery mica schists, and either to overlie or replace the upper portions of the banded gneiss series which succeeds to the fundamental series.

I do not propose to weary you further with the details of Alpine sections, except that I must add a few words upon the extent of this remarkable series to which I have now introduced you. On the northern side of the watershed in the Swiss Alps, so far as I am aware, it is not generally strongly developed, except in certain localities in the southernmost of the three ranges which make up the whole chain, but in parts of the Tyrol it is well displayed. It borders—the mica schists sometimes dominating—the fundamental gneiss in the Oetzthal *massif*; it forms the peak of the Gross Glockner; it meets us on the Brenner Pass and elsewhere overlain by and folded up with rocks which, if my memory do not mislead me, are the equivalents of the Lustrous Schists of more western districts.

Again, it is finely developed, seemingly in succession to bedded coarser gneiss, in some of the peaks of the Bernina range, and it occupies a considerable tract about the heads of the valleys to the south. It may be traced, indeed, over a great zone, and with but slight interruption all along the southern slopes of the Alps, even to the south of the head waters of the Po, forming many of the grandest peaks in the Graian, Tarentaise, Maurienne, and Cottian Alps; and we find traces of it overlying the coarse granitoid series in the *massif* of the Alps of Dauphiné.

Sections, indeed, in the neighbourhood of Biella, according to Gastaldi and Sterry Hunt, exhibit the Pietra Verde group overlying the upper or more bedded portion of the great gneissic or basal series, and succeeded by the group of friable gneisses, described above as closely associated with the garnetiferous schists, in a manner that suggests an unconformity. Under ordinary circumstances we should not hesitate to admit that there is considerable evidence in favour of this break, and some for one between the Pietra Verde group and the stronger gneisses and schists below; but in mountain regions we fear to trust our eyes. The evidence, however, in certain districts in favour of a break at the base of the Lustrous Schists is yet stronger. If I am right in regarding the Lustrous Schists as forming one group with the older part of the Bundnerschiefer of the Grisons region, and of the Thonschiefer of Von Hauer in the Eastern Alps, a study of the geological map will show that it is difficult to explain the relation of these beds to the underlying gneisses and schists without such an hypothesis. What I have myself seen in regard to the Lustrous Schists is strongly in favour of a great break in some localities. On the south side of the St. Gothard we have in the Val Piora the Lustrous Schists apparently in true succession with the representatives of the garnetiferous group of the Val Tremola, yet on the northern side, in the Urserenthal, the latter series is wanting, and the gneisses which underlie it appear to be immediately succeeded by the Lustrous Schists. This, however, might be explained by a complication of faulting and folding. What I have seen in the Binnenthal is harder to explain. At the head of the Hohsand Glacier, just below the peak of the Ofenhorn, we have a coarse but bedded gneiss, which I should correlate with the series immediately overlying the granitoid gneiss so often mentioned as the lowest rock of all. Glancing towards the north, across the snowfield, we see this rock in the base of the Strahlgrat distinctly overlain by the Lustrous series, with its characteristic band of limestone or dolomite. This series swoops down for some 2000 feet, and we cross it in the upper basin of the valley below, while yet further down the valley I detected the characteristic garnetiferous schist, of which, however, there is no great development. If this be the result of faulting and folding only, it is certainly very remarkable.

But I must linger no longer over details. The passing time warns me that I must attempt briefly to describe the general process of the building of this great mountain group of Europe. I have, I hope, proved that the metamorphic rocks of the Alps, if we may trust mineral similarity and mineral and lithological sequence, are vastly older than the Carboniferous period, and that in this ancient series a certain succession may be made out. If we may reason from the analogy of other regions, we may assign to the whole of their latest group (the Lustrous Schists) an antiquity greater than the earliest rocks in which indisputable traces of organic life have been found. One point, however, I should notice before proceeding further. It might perhaps be said—it has indeed been said—that the crystalline schists and gneisses of the Alps are the result of the great earth movements by which the mountains were upraised, when heat and pressure changed mud into schists and felspathic sandstone into gneiss. I have shown you that we can trace a mineral succession in the crystalline series of the Alpine chain, and that some at least of these are earlier than the Carboniferous period; but I can add to the proofs that these great rock masses had assumed in the main their present mineral structure when these movements occurred. We meet indeed with some rock masses whose structure is doubtless due to the pressure which they have undergone. This is the case with all cleaved rocks, as was lucidly explained, twenty-eight years since, by Professor Tyndall in this very room. We meet also with schists, where, from the arrangement of the mineral constituents, we have good reason for supposing that they were developed when the rock mass was exposed to a pressure definite in direction. Here the lines of different minerals, which we believe indicative of an original structure in the rock, are often wrinkled; the more flaky minerals commonly lie with their broader planes parallel, but, notwithstanding this, there is no very definite cleavage in the rock mass, nor tendency to separate easily along the different mineral layers. Specimens of such rocks may be obtained in the Alps, but there are others in which the layers have evidently been crumpled up after the period of mineral change: the bands of quartz and felspar have been, as it were, crushed together, the flakes of mica are sometimes crumbled and sometimes twisted round into new positions.

The subject is a technical one, so I must ask you to accept my statement, without the long details of microscopic work on which it is founded, that the older Alpine rocks frequently testify to having undergone an extraordinary amount of crushing. In the middle of coarse gneisses, for example, streaks and thin bands of a mica schist may be found, which are not due to an original difference of materials, but to the fact that here and there the original rock has yielded to enormous pressure, and has been crushed *in situ* into lenticular bands of rock dust, from which some new mineral developments have taken place. You may notice also in some regions, where you would classify the rocks at first sight as mica schists, that a close examination of the broken surfaces at right angles to what appear to be planes of foliation reveals a structure resembling a coarsish gneiss. The microscope shows that the rock is really a gneiss, somewhat crushed, and that the micaceous layers are of extreme tenuity—mere films, which do not seem to have been original constituents. The gneissic mass has been crushed, cleaved, and on the cleavage planes films of a hydro-mica have been developed. We cannot fail to be struck, when once our eyes have been opened to it, by the frequency of a slabby structure in the more central parts of the Alpine ranges, the surfaces of these slabs being coated with minute scales or films of mica. These are really records of a rude cleavage which has been impressed upon the more central and less flexible portions of the Alps during the great earth movements which they have undergone since they were first metamorphosed.

Thus in the building of the Alps our thoughts are carried very far back in the earth's history, far beyond the earliest strata of the Palæozoic age. Under what conditions were these great homogeneous granitoid masses of the fundamental gneisses formed? They differ on the one hand from granites, on the other from the ordinary gneisses; from the former their differences are but slight, and of uncertain value, yet into the latter they appear to graduate. There is nothing like to them in any subsequent rock group, and, so far as our knowledge at present goes, they appear to be the records of a period unique in the world's history. This may well be. When the dry land first appeared, when the surface of the earth's crust had not long ceased to glow, when the bulk of the ocean yet floated as a vapour in the heated atmosphere, when many gases now combined were free, we can well imagine that the earliest sediments would be deposited under conditions which have never been reproduced. In the later schists, with their more frequent mineral changes, their distinct stratification, and their beds of quartzite and of limestone, we may mark the gradual approach to a more normal condition of things. Some, such as the Lustrous Schists, may indeed be contemporaneous with our earliest Palæozoic rocks; but I confess that to myself the evidence appears more favourable to the idea that all are more ancient than the period which we call Cambrian, and that the majority are so I feel little doubt.

Supposing, then, that I am right in considering all the Alpine schists, even the Lustrous group, to be pre-Cambrian, we have a vast interval of time which has left no record in those districts of the Alps of which I have been speaking. It is not till we come to the Carboniferous period that we can identify any pages in the life history of the earth. We are justified with regard to these in the following conclusions:—

That in the place of the Alps there was at that time an upland district, composed of gneisses and schists, in substantially the same mineral condition as they are at present, together with slaty beds in a comparatively unaltered condition, which district was fringed by a lowland covered by a luxuriant vegetation. Prior to this time, also, the metamorphic rocks of the Alps had been so far folded and denuded that the coarser gneisses were in many places laid bare, and contributed the materials which we now find in such beds as the Val Orsine Pudding stone. Whether there was a pre-Triassic mountain chain occupying some part of the present Alpine region we cannot venture to say, but I think we may unhesitatingly affirm that there were pre-Triassic highlands.

After the close of the Carboniferous period, and anterior to the middle part of the Trias, there were volcanic outbursts on a large scale in more than one region of the Alps—notably in the district near and to the east of Botzen. After this commenced a period of subsidence and of continuous deposition of sediment. This seems to have begun earlier and to have been at first more rapid in the eastern than in the western area. Since in the former the Triassic beds are generally much thicker and more calcareous than in the latter, one is tempted to imagine that the eastern area quickly became a coraliferous sea, with an occasional atoll or volcanic island. Henceforward to the later part of the Eocene the record is generally one of subsidence and of deposit of sediment. Pebble beds are rare: the strata are grits, shales (or slates), and limestones. Whence the inorganic constituents of these were derived I cannot at present venture to suggest, but though conglomerates are rare, there are occasional indications that land was not very distant. In the eastern Alps, however, the position of some of the Cretaceous deposits and the marked mineral differences between these and the Jurassic seem to indicate disturbances during some part of the Neocomian, but I am not aware of any marked trace of these over the central and western areas. The mountain-making of the existing Alps dates from the later part of the Eocene. Beds of about the age of our Bracklesham series now cap such summits as the Diablerets, or help to form the mountain masses near the Tödi, rising in the Bifertenstock to a height of 11,300 feet above the sea. Still there are signs that the sea was shallowing and the epoch of earth movements commencing. The Eocene deposits of Switzerland include terrestrial and fluviatile, as well as marine remains. Beds of conglomerate occur, and even erratics of a granite from an unknown locality, of such a size as to suggest the aid of ice for their transport. For the present I prefer, for sake of simplicity, to speak of the upraising of the Alps as though it were the result of a few acts of compression, though I am by no means sure that this is the case. Thus speaking we find that in Miocene times a great mountain chain existed which covered nearly the same ground as the present Alpine region of mesozoic and crystalline rocks. To the north, and probably to the south, lay shallow seas, between which and the gates of the hills was a level tract traversed by rivers, perhaps in part occupied by lakes. Over this zone, as it slowly subsided—in correspondence, probably, with the uplifting of the mountain land—were deposited the pebble beds of the nagelflue and the sandstones of the molasse.

Then came another contraction of the earth's crust; the solid mountain core was no doubt compressed, uplifted, and thrust over newer beds, but the region of the softer border land, at any rate on the north, was apparently more affected, and the subalpine district of Switzerland was the result. I may here call your attention to the fact that, whether as a consequence of this or of subsequent movements, the miocene beds occur on the northern flank of the Alps at a much greater height above the sea than on the southern, and have been much more upraised in the central than in the western and eastern Alps. Further, between the Lago Maggiore and the south of Saluzzo mesozoic rocks are almost absent from the southern flank of the Alps, and the miocene beds are but slightly exposed and occupy a comparatively lowland country. I think it therefore probable that the second set of movements produced more effect on the German than on the Italian side of the Alps, causing in the latter a relative depression. In support of this view we may remark that the rivers which flow from the Alps towards the north or the west start, as a rule, very far back, so that the watershed of the Alps is the crest of the third range reckoning from the north, and the great flat basin of the Po is the receptacle for a series of comparatively short mountain rivers. These also take a fairly straight course to the gates of the hills, while the others change not seldom from the lines of outcrop to the lines of dip of the strata—a fact I think not without significance. To this rule the valley of the Adige in the eastern region is an exception. May not this be due to the remarkable series of minor flexures indicated by the strike of the rocks (secondary and earlier) immediately to the west of it, which probably influences the course of the Adda and can, I think, be traced at intervals along the chain as far as Dauphiné? Be this as it may, it is obvious that the generally uniform E.N.E. to W.S.W. strike of the rocks which compose the Alpine chain is materially modified as we proceed south of the lake of Geneva, changing rapidly in the neighbourhood of Grenoble from a strike N.E. to S.W. to one from N.W. to S.E. This subject, however, is too complicated to be followed further on the present occasion. I will only add that the singular trough-like upland valleys, forming the upper parts of some of the best-known road passes—as, for instance, the Maloya—which descend so gently to the north, and are cut off so abruptly on the south, seem to me most readily explained as the remnants of a comparatively disused drainage system of the Alps.

It remains only to say a few words on the post-tertiary history of the Alps. We enter here upon a troubled sea of controversy, upon which more than the time during which I have spoken might easily be spent; so you will perhaps allow me to conclude with a simple expression of my own opinion, without entering into the arguments. That the glaciers of the Alps were once vastly greater than at the present time is beyond all dispute; they covered the fertile lowlands of Switzerland, they welled up against the flanks of the Jura above Neufchatel, they crept over the orange gardens of Sirmio, and projected into the plains of Piedmont. By their means great piles of broken rock must have been transported into the lowlands; but did they greatly modify the peaks, deepen the valleys, or excavate the lake basins? My reply would be, "To no very material extent." I regard the glacier as the file rather than as the chisel of nature. The Alpine lakes appear to be more easily explained, as the Dead Sea can only be explained—as the result of subsidence along zones roughly parallel with the Alpine ranges, athwart the general directions of valleys which already existed and had been in the main completed in preglacial times. To produce these lake basins we should require earth movements on no greater scale than have taken place in our own country since the furthermost extension of the ice-fields. This opinion as to the origin of the lakes is, I believe, generally held to be a heresy, but it is a heresy which has been ingrained in me by some twenty years of study of the physiography of the Alps.

[T. G. B.]

GENERAL MONTHLY MEETING,

Monday, April 7, 1884.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Robert Ellis Dudgeon, M.D.
David John Russell Duncan, Esq.
Willoughby Smith, Esq.

were elected Members of the Royal Institution.

Four Candidates for Membership were proposed for election.

The Fullerian Professorship of Physiology became vacant by the resignation of Professor McKendrick on March 5th, on account of ill health.

The following arrangements for the Lectures after Easter were announced:—

Edward E. Klein, M.D. F.R.S. and Professor Arthur Gamgee, M.D. F.R.S.—Seven Lectures on The Anatomy and Physiology of Nerve and Muscle. Dr. Klein.—Two Lectures on The Anatomy of Nerve and Muscle; on Tuesdays, April 22 and 29. Professor Gamgee.—Five Lectures on The Physiology of Nerve and Muscle; on Tuesdays, May 6 to June 3.

Professor Dewar, M.A. F.R.S. *M.R.I.*—Seven Lectures on Flame and Oxidation; on Thursdays, April 24 to June 5.

Hodder M. Westropp, Esq.—Three Lectures on Recent Discoveries in Roman Archæology: I. The Colosseum; II. The Forum; III. The Palatine Hill; on Saturdays, April 26 to May 10.

Professor T. G. Bonney, D.Sc. F.R.S. Pres. G.S.—Four Lectures on The Bearing of Microscopical Research upon some large Geological Problems; on Saturdays, May 17 to June 7.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

The Governor-General of India—Geological Survey of India: Palæontologia Indica: Series X. Vol. III. Part 1. 4to. 1884.
Records, Vol. XVII. Part 1. 8vo. 1884.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Transunti. Vol. VIII. Fasc. 4–6. 4to. 1884.
Asiatic Society of Bengal—Proceedings, No. 9. 8vo. 1883.
Journal, Vol. LII. Part I. Nos. 3, 4; Part II. Nos. 2–4. 8vo. 1883.
Astronomical Society, Royal—Monthly Notices, Vol. XLIV. No. 4. 8vo. 1884.
Bankers, Institute of—Journal, Vol. V. Part 3. 8vo. 1884.

Chemical Society—Journal for March 1884. 8vo.
Editors—American Journal of Science for March 1884. 8vo.
  Analyst for March 1884. 8vo.
  Athenæum for March 1884. 4to.
  Chemical News for March 1884. 4to.
  Engineer for March 1884. fol.
  Horological Journal for March 1884. 8vo.
  Iron for March 1884. 4to.
  Nature for March 1884. 4to.
  Revue Scientifique and Revue Politique et Littéraire for March 1884. 4to.
  Science Monthly, Illustrated, for March, 1884.
  Telegraphic Journal for March 1884. 8vo.
Franklin Institute—Journal, No. 699. 8vo. 1884.
Geographical Society, Royal—Proceedings, New Series, Vol. VI. No. 3. 8vo. 1884.
Geological Institute, Imperial, Vienna—Jahrbuch, Band XXXIV. No. 1. 8vo. 1884.
Johns Hopkins University—American Chemical Journal, Vol. VI. No. 1. 8vo. 1884.
  American Journal of Philology, No. 16. 8vo. 1883.
Linnean Society—Journal, No. 102. 8vo. 1884.
Lisbon, Sociedade de Geographia—Bulletin, 4e Serie, Nos. 4, 5. 8vo. 1883.
Manchester Geological Society—Transactions, Vol. XVII. Parts 13, 14. 8vo. 1883–4.
Mechanical Engineers' Institution—Proceedings, No. 1. 8vo. 1884.
Middlesex Hospital—Reports for 1881. 8vo. 1884.
Miller, W. J. C. Esq. (the Registrar)—The Medical Register. 8vo. 1884.
  The Dentists' Register. 8vo. 1884.
Newlands, John A. R. Esq. F.I.C. F.C.S. (the Author)—The Periodic Law. 8vo. 1884.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXIII. Part 3. 8vo. 1884.
Numismatic Society—Chronicle and Journal, 1883, Part 4. 8vo.
Pharmaceutical Society of Great Britain—Journal, March 1884. 8vo.
Photographic Society—Journal, New Series, Vol. VIII. No. 5. 8vo. 1884.
Radcliffe Observatory—Radcliffe Observations for 1858–80. 8vo.
  Radcliffe Catalogue of Stars. 2 vols. 8vo. 1860.
Rio de Janeiro, Observatoire Imperiale—Bulletin, No. 10. fol. 1883.
Royal College of Surgeons of England—Catalogue of Specimens illustrating the Osteology of Vertebrated Animals in the Museum. By W. H. Flower. Vol. 2, Mammalia. 8vo. 1884.
Royal Society of London—Proceedings, No. 229. 8vo. 1884.
St. Pétersbourg, Académie des Sciences—Mémoires, Tome XXXI. No. 9. 4to. 1883.
Saxon Society of Sciences, Royal—Philologisch-historische Classe: Abhandlungen: Band VIII. Nos. 5, 6; Band IX. No. 1. 8vo. 1883.
  Verhandlungen, 1882. 8vo. 1883.
  Mathematisch-physische Classe: Abhandlungen: Band XII. No. 9. 8vo. 1883.
  Verhandlungen, 1882. 8vo. 1883.
Society of Arts—Journal, March 1884. 8vo.
Tokio University—Memoirs, No. 9. 8vo. 1884.
Upsal University—Nova Acta, Ser. III. Vol. XI. Fasc. 2. 4to. 1883.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1884: Heft 2. 4to.
Yorkshire Archæological and Topographical Association—Journal, Part 31. 8vo. 1884.

WEEKLY EVENING MEETING,

Friday, April 25, 1884.

Sir Frederick Pollock, Bart. M.A. Manager and Vice-President, in the Chair.

Walter Besant, Esq.

The Art of Fiction.[*]

I desire, this evening, to consider Fiction as one of the Fine Arts. In order to do this, and before doing it, I have first to advance certain propositions. They are not new, they are not likely to be disputed, and yet they have never been so generally received as to form part, so to speak, of the national mind. These propositions are three, though the last two directly spring from the first. They are:—

1. That Fiction is an Art in every way worthy to be called the sister and the equal of the Arts of Painting, Sculpture, Music and Poetry; that is to say, her field is as boundless, her possibilities as vast, her excellences as worthy of admiration, as may be claimed for any of her sister Arts.
2. That it is an Art which, like them, is governed and directed by general laws; and that these laws may be laid down and taught with as much precision and exactness as the laws of harmony, perspective, and proportion.
3. That, like the other Fine Arts, Fiction is so far removed from the mere mechanical arts, that no laws or rules whatever can teach it to those who have not already been endowed with the natural and necessary gifts.

These are the three propositions which I have to discuss. It follows as a corollary and evident deduction, that, these propositions once admitted, those who follow and profess the Art of Fiction must be recognised as artists, in the strictest sense of the word, just as much as those who have delighted and elevated mankind by music and painting; and that the great Masters of Fiction must be placed on the same level as the great Masters in the other Arts. In other words, I mean that where the highest point, or what seems the highest point, possible in this Art is touched, the man who has reached it is one of the world's greatest men. . . . .

The general—the Philistine—view of the Profession, is, first of all, that it is not one which a scholar and a man of serious views should take up: the telling of stories is inconsistent with a well-balanced mind; to be a teller of stories disqualifies one from a hearing on important subjects. . . . .

---

[*] The full discourse is published by Messrs. Chatto and Windus.

With these people must not be confounded another class, not so large, who are prepared to admit that Fiction is in some qualified sense an Art; but they do this as a concession to the vanity of its followers, and are by no means prepared to allow that it is an Art of the first rank. How can that be an Art, they might ask, which has no lecturers or teachers, no school or college or Academy, no recognised rules, no text-books, and is not taught in any University? Even the German Universities, which teach everything else, do not have Professors of Fiction, and not one single novelist, so far as I know, has ever pretended to teach his mystery, or spoken of it as a thing which may be taught. Clearly, therefore, they would go on to argue, such art as is required for the making and telling of a story can and must be mastered without study, because no materials exist for the student's use. It may even, perhaps, be acquired unconsciously, or by imitation. This view, I am sorry to say, largely prevails among the majority of those who try their chance in the field of fiction. Anyone, they think, can write a novel; therefore, why not sit down and write one? I would not willingly say one word which might discourage those who are attracted to this branch of literature; on the contrary, I would encourage them in every possible way. One desires, however, that they should approach their work at the outset with the same serious and earnest appreciation of its importance and its difficulties with which they undertake the study of music and painting. I would wish, in short, that from the very beginning their minds should be fully possessed with the knowledge that Fiction is an Art, and, like all other Arts, that it is governed by certain laws, methods, and rules, which it is their first business to learn.

It is, then, first and before all, a real Art. It is the oldest, because it was known and practised long before Painting and her sisters were in existence or even thought of; it is older than any of the Muses from whose company she who tells stories has hitherto been excluded; it is the most widely spread, because in no race of men under the sun is it unknown, even though the stories may be always the same, and handed down from generation to generation in the same form; it is the most religious of all the Arts, because in every age until the present the lives, exploits and sufferings of gods, goddesses, saints and heroes have been the favourite theme; it has always been the most popular, because it requires neither culture, education, nor natural genius to understand and listen to a story; it is the most moral, because the world has always been taught whatever little morality it possesses by way of story, fable, apologue, parable, and allegory. It commands the widest influence, because it can be carried easily and everywhere, into regions where pictures are never seen and music is never heard; it is the greatest teaching power, because its lessons are most readily apprehended and understood. All this, which might have been said thousands of years ago, may be said to day with even greater force and truth. That world which exists not, but is an invention or an imitation—that world in which the shadows and shapes of men move about before our eyes as real as if they were actually living and speaking among us, is like a great theatre accessible to all of every sort, on whose stage are enacted, at our own sweet will, whenever we please to command them, the most beautiful plays: it is, as every theatre should be, the school in which manners are learned: here the majority of reading mankind learn nearly all that they know of life and manners, of philosophy and art; even of science and religion. The modern novel converts abstract ideas into living models; it gives ideas, it strengthens faith, it preaches a higher morality than is seen in the actual world; it commands the emotions of pity, admiration, and terror; it creates and keeps alive the sense of sympathy; it is the universal teacher; it is the only book which the great mass of reading mankind ever do read; it is the only way in which people can learn what other men and women are like; it redeems their lives from dulness, puts thoughts, desires, knowledge, and even ambitions into their hearts; it teaches them to talk, and enriches their speech with epigrams, anecdotes and illustrations. It is an unfailing source of delight to millions, happily not too critical. Why, out of all the books taken down from the shelves of the public libraries, four-fifths are novels, and of all those that are bought nine-tenths are novels. Compared with this tremendous engine of popular influence, what are all the other Arts put together? Can we not alter the old maxim, and say with truth, Let him who pleases make the laws if I may write the novels?

As for the field with which this Art of Fiction occupies itself, it is, if you please, nothing less than the whole of Humanity. The novelist studies men and women; he is concerned with their actions and their thoughts, their errors and their follies, their greatness and their meanness; the countless forms of beauty and constantly varying moods to be seen among them; the forces which act upon them; the passions, prejudices, hopes and fears which pull them this way and that. He has to do, above all and before all, with men and women. No one, for instance, among novelists, can be called a landscape painter, or a painter of sea-pieces, or a painter of fruit and flowers, save only in strict subordination to the group of characters with whom he is dealing. . . . .

It is, therefore, the especial characteristic of this Art, that, since it deals exclusively with men and women, it not only requires of its followers, but also creates in readers, that sentiment which is destined to be a most mighty engine in deepening and widening the civilization of the world. We call it Sympathy, but it means a good deal more than was formerly understood by the word. It means, in fact, what Professor Seeley once called the Enthusiasm of Humanity, and it first appeared, I think, about a hundred and fifty years ago, when the modern novel came into existence. You will find it, for instance, conspicuous for its absence in Defoe. The modern Sympathy includes not only the power to pity the sufferings of others, but also that of understanding their very souls; it is the reverence for man, the respect for his personality, the recognition of his individuality, and the enormous value of the one man, the perception of one man's relation to another, his duties and responsibilities. Through the strength of this newly-born faculty, and aided by the guidance of a great artist, we are enabled to discern the real indestructible man beneath the rags and filth of a common castaway, and the possibilities of the meanest gutter child that steals in the streets for its daily bread. Surely that is a wonderful Art which endows the people—all the people—with this power of vision and of feeling. Painting has not done it, and could never do it; Painting has done more for nature than for humanity. Sculpture could not do it, because it deals with situation and form, rather than action. Music cannot do it, because Music (if I understand rightly) appeals especially to the individual concerning himself and his own aspirations. Poetry alone is the rival of Fiction, and in this respect it takes a lower place, not because Poetry fails to teach and interpret, but because Fiction is, and must always be, more popular.

Again, this Art teaches, like the others, by suppression and reticence. Out of the great procession of Humanity, the *Comédie Humaine*, which the novelist sees passing ever before his eyes, single figures detach themselves one after the other, to be questioned, examined, and received or rejected. This process goes on perpetually. Humanity is so vast a field, that to one who goes about watching men and women, and does not sit at home and evolve figures out of inner consciousness, there is not and can never be any end or limit to the freshness and interest of these figures. It is the work of the artist to select the figures, to suppress, to copy, to group, and to work up the incidents which each one offers. The daily life of the world is not dramatic—it is monotonous; the novelist makes it dramatic by his silences, his suppressions, and his exaggerations. No one, for example, in fiction behaves quite in the same way as in real life; as on the stage, if an actor unfolds and reads a letter, the simple action is done with an exaggeration of gesture which calls attention to the thing and to its importance, so in romance, while nothing should be allowed which does not carry on the story, so everything as it occurs must be accentuated and yet deprived of needless accessory details. The gestures of the characters at an important juncture, their looks, their voices, may all be noted if they help to impress the situation. Even the weather, the wind, and the rain, with some writers, have been made to emphasize a mood or a passion of a heroine. To know how to use these aids artistically is to the novelist exactly what to the actor is the right presentation of a letter, the handing of a chair, even the removal of a glove.

A third characteristic of Fiction, which should alone be sufficient to give it a place among the noblest forms of Art, is that, like

Poetry, Painting, and Music, it becomes a vehicle, not only for the best thoughts of the writer, but also for those of the reader, so that a novelist may write truthfully and faithfully, but simply, and yet be understood in a far fuller and nobler sense than was present to his own mind. This power is the very highest gift of the poet. He has a vision and sees a thing clearly, yet perhaps afar off; another who reads him is enabled to get the same vision, to see the same thing, yet closer and more distinctly. For a lower intellect thus to lead and instruct a higher is surely a very great gift, and granted only to the highest forms of Art. And this it is which Fiction of the best kind does for its readers. It is, however, only another way of saying that Truth in Fiction produces effects similar to those produced by Truth in every other Art. . . . .

We come next to speak of the Laws which govern this Art. I mean those general rules and principles which must necessarily be acquired by every writer of Fiction before he can even hope for success. Rules will not make a man a novelist, any more than a knowledge of grammar makes a man know a language, or a knowledge of musical science makes a man able to play an instrument. Yet the Rules must be learned. And, in speaking of them, one is compelled, so close is the connection between the sister Arts, to use not only the same terms, but also to adopt the same rules, as those laid down by painters for their students. If these Laws appear self-evident, it is a proof that the general principles of the Art are well understood. Considering, however, the vast quantity of bad, inartistic work which is every week laid before the public, one is inclined to think that a statement of these principles may not be without usefulness.

First, and before everything else, there is the Rule that everything in Fiction which is invented and is not the result of personal experience and observation is worthless. In some other Arts, the design may follow any lines which the designer pleases: it may be fanciful, unreal, or grotesque; but in modern Fiction, whose sole end, aim, and purpose is to portray humanity and human character, the design must be in accordance with the customs and general practice of living men and women under any proposed set of circumstances and conditions. That is to say, the characters must be real, and such as might be met with in actual life, or, at least, the natural developments of such people as any of us might meet; their actions must be natural and consistent; the conditions of place, of manners, and of thought must be drawn from personal observation. . . . . Remember that most of the people who read novels and know nothing about the art of writing them, recognise before any other quality that of fidelity: the greatness of a novelist they measure chiefly by the knowledge of the world displayed in his pages; the highest praise they can bestow upon him is that he has drawn the story to the life. . . . .

This being so, the first thing which has to be acquired is the art of description. It seems easy to describe; anyone, it seems, can set down what he sees. But consider. How much does he see? There is everywhere, even in a room, such a quantity of things to be seen: far, far more in field and hedge, in mountain and in forest, and beside the stream are there countless things to be seen; the unpractised eye sees nothing, or next to nothing. Here is a tree, here is a flower, there is sunshine lying on the hill. But to the observant and trained eye, the intelligent eye, there lies before him everywhere an inexhaustible and bewildering mass of things to see. Remember how Mr. Jefferies sits down in a coppice with his eyes wide open to see what the rest of us never dreamed of looking for. Long before he has half finished telling us what he has seen—behold! a volume, and one of the most delightful volumes conceivable. But then, Mr. Jefferies is a profound naturalist. We cannot all describe after his manner; nor should we try, for the simple reason that descriptions of still life in a novel must be strictly subordinated to the human interest. But while Mr. Jefferies has his hedge and ditch and brook, we have our towns, our villages, and our assemblies of men and women. Among them we must not only observe, but we must select. Here, then, are two distinct faculties which the intending novelist must acquire; viz. observation and selection. As for the power of observation, it may be taught to anyone by the simple method adopted by Robert Houdin, the French conjuror. This method consists of noting down continually and remembering all kinds of things remarked in the course of a journey, a walk, or the day's business. The learner must carry his note-book always with him, into the fields, to the theatre, into the streets—wherever he can watch man and his ways, or Nature and her ways. On his return home he should enter his notes in his commonplace-book. There are places where the production of a note-book would be embarrassing—say, at a dinner-party, or a street fight; yet the man who begins to observe will speedily be able to remember everything that he sees and hears until he can find an opportunity to note it down, so that nothing is lost.* The materials for the novelist, in short, are not in the books upon the shelves, but in the men and women he meets with everywhere; he will find them, where Dickens found them, in the crowded streets, in trains, tramcars and omnibuses, at the shop-windows, in churches and chapels: his materials are everywhere—there is nothing too low, nothing too high, nothing too base, nothing too noble, for the novelist. Humanity is like a kaleidoscope, which you may turn about and look into, but you will never get the same picture twice—it cannot be exhausted. But it may be objected, that the broad distinctive types have been long since all used. They *have* been used, but the comfort is that they can never be used up, and that they may be constantly used again and again. Can we ever be tired of them when a master hand takes one of them again and gives him new life? . . . .

---

* I earnestly recommend those who desire to study this Art to begin by daily practice in the description of things, even common things, that they have observed, by reporting conversations, and by word portraits of their friends. They will find that the practice gives them firmness of outline, quickness of observation, power of catching important details, and, as regards dialogue, readiness to see what is unimportant. Preliminary practice and study of this kind will also lead to the saving of a vast quantity of valuable material, which is only wasted by being prematurely worked up into a novel written before the elements of the Art have been acquired.

Fidelity, therefore, can be only assured by acquiring the art of observation, which further assists in filling the mind with stored experience. I am quite sure that most men never see anything at all. I have known men who have even gone all round the world and seen nothing—no, nothing at all. Emerson says, very truly, that a traveller takes away nothing from a place except what he brought into it. Now, the observation of things around us is no part of the ordinary professional and commercial life; it has nothing at all to do with success and the making of money; so that we do not learn to observe. Yet it is very easy to shake people and make them open their eyes. Some of us remember, for instance, the time when Kingsley astonished everybody with his descriptions of the wonders to be seen on the seashore and to be fished out of every pond in the field. Then all the world began to poke about the seaweed and to catch tritons and keep water-grubs in little tanks. It was only a fashion, and it presently died out; but it did people good, because it made them understand, perhaps for the first time, that there really is a good deal more to see than meets the casual eye. At present the lesson which we need is not that the world is full of the most strange and wonderful creatures, all eating each other perpetually, but that the world is full of the most wonderful men and women, not one of whom is mean or common, but to each his own personality is a great and awful thing, worthy of the most serious study.

There are, then, abundant materials waiting to be picked up by anyone who has the wit to see them lying at his feet and all around him. What is next required is the power of Selection. Can this be taught? I think not, at least I do not know how, unless it is by reading. In every Art, selection requires that kind of special fitness for the Art which is included in the much-abused word Genius. In Fiction, the power of selection requires a large share of the dramatic sense. Those who already possess this faculty will not go wrong if they bear in mind the simple rule that nothing should be admitted which does not advance the story, illustrate the characters, bring into stronger relief the hidden forces which act upon them, their emotions, their passions, and their intentions. All descriptions which hinder instead of helping the action, all episodes of whatever kind, all conversation which does not either advance the story or illustrate the characters, ought to be rigidly suppressed.

Closely connected with selection is dramatic presentation. Given a situation, it should be the first care of the writer to present it as dramatically, that is to say, as forcibly as possible. The grouping and setting of the picture, the due subordination of description to dialogue, the rapidity of the action, those things which naturally suggest themselves to the practised eye, deserve to be very carefully considered by the beginner. In fact, a novel is like a play: it may be divided into scenes and acts, tableaux and situations, separated by the end of the chapter instead of the drop scene: the writer is the dramatist, stage-manager, scene-painter, actor, and carpenter, all in one: it is his single business to see that none of the scenes flag or fall flat: he must never for one moment forget to consider how the piece is looking from the front.

The next simple Rule is that the drawing of each figure must be clear in outline, and, even if only sketched, must be sketched without hesitation. This can only be done when the writer himself sees his figures clearly. Characters in fiction do not, it must be understood, spring Minerva-like from the brain. They grow: they grow sometimes slowly, sometimes quickly. From the first moment of conception, that is to say, from the first moment of their being seen and caught, they grow continuously and almost without mental effort. If they do not grow and become every day clearer, they had better be put aside at once, and forgotten as soon as may be, because that is a proof that the author does not understand the character he has himself endeavoured to create. To have on one's hands a half-created being without the power of finishing him must be a truly dreadful thing. The only way out of it is to kill and bury him at once. . . . . . On the other hand, how possible, how capable of development, how real becomes a true figure, truly understood by the creator, and truly depicted! Do we not know what they would say and think under all conceivable conditions? We can dress them as we will; we can place them in any circumstances of life: we can always trust them, because they will never fail us, never disappoint us, never change, because we understand them so thoroughly. So well do we know them that they become our advisers, our guides, and our best friends, on whom we model ourselves, our thoughts, and our actions. The writer who has succeeded in drawing to the life, true, clear, distinct, so that all may understand, a single figure of a true man or woman, has added another exemplar or warning to humanity. Nothing, then, it must be insisted upon as of the greatest importance, should be begun in writing until the characters are so clear and distinct in the brain, so well known, that they will act their parts, bend their dialogue, and suit their action to whatever situations they may find themselves in, if only they are becoming to them. Of course, clear outline drawing is best when it is accomplished in the fewest strokes, and the greater part of the figures in Fiction, wherein it differs from Painting, in which everything should be finished, require no more work upon them, in order to make them clear, than half-a-dozen bold, intelligible lines.

As for the methods of conveying a clear understanding of a character, they are many. The first and the easiest is to make it clear by reason of some mannerism or personal peculiarity, some trick of speech or of carriage. This is the worst, as may generally be said of the easiest way. Another easy method is to describe your character at length. This also is a bad, because a tedious, method. If, however, you read a page or two of any good writer, you will discover that he first makes a character intelligible by a few words, and then allows him to reveal himself in action and dialogue. On the other hand, nothing is more inartistic than to be constantly calling attention in a dialogue to a gesture or a look, to laughter or to tears. The situation generally requires no such explanation: in some well-known scenes which I could quote, there is not a single word to emphasize or explain the attitude, manner, and look of the speakers, yet they are as intelligible as if they were written down and described. That is the highest art which carries the reader along and makes him see, without being told, the changing expressions, the gestures of the speakers, and hear the varying tones of their voices. It is as if one should close one's eyes at the theatre, and yet continue to see the actors on the stage as well as hear their voices. The only writer who can do this is he who makes his characters intelligible from the very outset, causes them first to stand before the reader in clear outline, and then with every additional line brings out the figure, fills up the face, and makes his creatures grow from simple outline more and more to the perfect and rounded figure.

Clearness of drawing, which includes clearness of vision, also assists in producing directness of purpose. As soon as the actors in the story become real in the mind of the narrator, and not before, the story itself becomes real to him. More than this, he becomes straightway vehemently impelled to tell it, and he is moved to tell it in the best and most direct way, the most dramatic way, the most truthful way possible to him. It is, in fact, only when the writer believes his own story, and knows it to be every word true, and feels that he has somehow learned from everyone concerned the secret history of his own part in it, that he can really begin to write it.* We know how sometimes, even from a practised hand, there comes a work marred with the fatal defect that the writer does not believe in his own story. When this is the case, one may generally find on investigation that one cause at least of the failure is that the characters, or some of them, are blurred and uncertain.

Again, the modern English novel, whatever form it takes, almost always starts with a conscious moral purpose. When it does not, so much are we accustomed to expect it, that one feels as if there has been a debasement of the Art. It is, fortunately, not possible in this country for any man to defile and defame humanity and still be called an artist; the development of modern sympathy, the growing reverence for the individual, the ever-widening love of things beautiful and the appreciation of lives made beautiful by devotion and self-denial, the sense of personal responsibility among the English-speaking races, the deep-seated religion of our people, even in a time of doubt, are all forces which act strongly upon the artist as well as upon his readers, and lend to his work, whether he will or not, a moral purpose so clearly marked that it has become practically a law of English Fiction. We must acknowledge that this is a truly admirable thing, and a great cause for congratulation. At the same time, one may be permitted to think that the preaching novel is the least desirable of any, and to be unfeignedly rejoiced that the old religious novel, written in the interests of High Church or Low Church or any other Church, has gone out of fashion.

---

* Hardly anything is more important than this—to believe in your own story. Wherefore let the student remember that unless the characters exist and move about in his brain, all separate, distinct, living, and perpetually engaged in the action of the story, sometimes at one part of it, sometimes at another, and that in scenes and places which must be omitted in the writing, he has got no story to tell and had better give it up. I do not think it is generally understood that there are thousands of scenes which belong to the story and never get outside the writer's brain at all. Some of these may be beautiful and touching; but there is not room for all, and the writer has to select.

Next, just as in Painting and Sculpture, not only are fidelity, truth, and harmony to be observed in Fiction, but also beauty of workmanship. It is almost impossible to estimate too highly the value of careful workmanship, that is, of style. Every one, without exception, of the great Masters in Fiction, has recognised this truth. You will hardly find a single page in any of them which is not carefully and even elaborately worked up. I think there is no point on which critics of novels should place greater importance than this, because it is one which young novelists are so very liable to ignore. There ought not to be in a novel, any more than in a poem, a single sentence carelessly worded, a single phrase which has not been considered. Consider, if you please, any one of the great scenes in Fiction—how much of the effect is due to the style, the balanced sentences, the very words used by the narrator! This, however, is only one more point of similarity between Fiction and the sister Arts. There is, I know, the danger of attaching too much attention to style at the expense of situation, and so falling a prey to priggishness, fashions, and mannerisms of the day. It is certainly a danger; at the same time, it sometimes seems, when one reads the slipshod, careless English which is often thought good enough for story-telling, that it is almost impossible to overrate the value of style. There is comfort in the thought that no reputation worth having can be made without attending to style, and that there is no style, however rugged, which cannot be made beautiful by attention and pains. . . . . .

In fact, every scene, however unimportant, should be completely and carefully finished. There should be no unfinished places, no sign anywhere of weariness or haste—in fact, no scamping. The writer must so love his work as to dwell tenderly on every page and be literally unable to send forth a single page of it without the finishing touches. We all of us remember that kind of novel in which every scene has the appearance of being hurried and scamped.

To sum up these few preliminary and general laws. The Art of Fiction requires first of all the power of description, truth, and fidelity, observation, selection, clearness of conception and of outline, dramatic grouping, directness of purpose, a profound belief on the part of the story-teller in the reality of his story, and beauty of workmanship. It is, moreover, an Art which requires of those who follow it seriously that they must be unceasingly occupied in studying the ways of mankind, the social laws, the religions, philosophies, tendencies, thoughts, prejudices, superstitions of men and women. They must consider as many of the forces which act upon classes and upon individuals as they can discover; they should be always trying to put themselves into the place of another; they must be as inquisitive and as watchful as a detective, as suspicious as a criminal lawyer, as eager for knowledge as a physicist, and withal fully possessed of that spirit to which nothing appears mean, nothing contemptible, nothing unworthy of study, which belongs to human nature.

I repeat that I submit some of these laws as perhaps self-evident. If that is so, many novels which are daily submitted to the reviewer are written in wilful neglect and disobedience of them. But they are not really self-evident; those who aspire to be artists in Fiction almost invariably begin without any understanding at all of these laws. Hence the lamentable early failures, the waste of good material, and the low level of Art with which both the novel-writer and the novel-reader are too often contented. I am certain that if these laws were better known and more generally studied, a very large proportion of the bad works of which our critics complain would not be produced at all. And I am in great hopes that one effect of the establishment of the newly founded Society of Authors will be to keep young writers of fiction from rushing too hastily into print, to help them to the right understanding of their Art and its principles, and to guide them into true practice of their principles while they are still young, their imaginations strong, and their personal experiences as yet not wasted in foolish failures.

After all these preliminary studies there comes the most important point of all—the story. There is a school which pretends that there is no need for a story: all the stories, they say, have been told already; there is no more room for invention: nobody wants any longer to listen to a story. One hears this kind of talk with the same wonder which one feels when a new monstrous fashion changes the beautiful figure of woman into something grotesque and unnatural. Men say these things gravely to each other, especially men who have no story to tell: other men listen gravely; in the same way women put on the newest and most preposterous fashions gravely, and look upon each other without either laughing or hiding their faces for shame. It is indeed, if we think of it, a most strange and wonderful theory, that we should continue to care for Fiction and cease to care for the story. We have all along been training ourselves how to tell the story, and here is this new school which steps in like the needy knife-grinder, to explain that there is no story left at all to tell. Why, the story is everything. I cannot conceive of a world going on at all without stories, and those strong ones, with incident in them, and merriment and pathos, laughter, and tears and the excitement of wondering what will happen next. Fortunately, these new theorists contradict themselves, because they find it impossible to write a novel which shall not contain a story, although it may be but a puny bantling. Fiction without adventure—a drama without a plot—a novel without surprises—the thing is as impossible as life without uncertainty.

As for the story, then. And here theory and teaching can go no farther. For every Art there is the corresponding science which may be taught. We have been speaking of the corresponding science. But the Art itself can neither be taught nor communicated. If the thing is in a man he will bring it out somehow, well or badly, quickly or slowly. If it is not, he can never learn it. Here, then, let us suppose that we have to do with the man to whom the invention of stories is part of his nature. We will also suppose that he has mastered the laws of his Art, and is now anxious to apply them. To such a man one can only recommend that he should with the greatest care and attention analyze and examine the construction of certain works which are acknowledged to be of the first rank in fiction. Among them, not to speak of Scott, he might pay especial attention from the constructive point of view, to the truly admirable shorter stories of Charles Reade, to George Eliot's 'Silas Marner,' the most *perfect* of English novels, Hawthorne's 'Scarlet Letter,' Holmes's 'Elsie Venner,' Blackmore's 'Lorna Doone,' or Black's 'Daughter of Heth.' He must not sit down to read them "for the story," as uncritical people say: he must read them slowly and carefully, perhaps backwards, so as to discover for himself how the author built up the novel, and from what original germ or conception it sprang. . . . .

One thing more the Art student has to learn. Let him not only believe his own story before he begins to tell it, but let him remember that in story-telling, as in almsgiving, a cheerful countenance works wonders, and a hearty manner greatly helps the teller and pleases the listener. One would not have the novelist make continual efforts at being comic; but let him not tell his story with eyes full of sadness, a face of woe and a shaking voice. His story may be tragic, but continued gloom is a mistake in Art, even for a tragedy. If his story is a comedy, all the more reason to tell it cheerfully and brightly. Lastly, let him tell it without apparent effort: without trying to show his cleverness, his wit, his powers of epigram, and his learning. Yet let him pour without stint or measure into his work all that he knows, all that he has seen, all that he has observed, and all that he has remembered: all that there is of nobility, sympathy, and enthusiasm in himself. Let him spare nothing, but lavish all that he has, in the full confidence that the wells will not be dried up, and that the springs of fancy and imagination will flow again, even though he seem to have exhausted himself in this one effort. . . . . .

Let me say one word upon the present condition of this most delightful Art in England. Remember that great Masters in every Art are rare. Perhaps one or two appear in a century: we ought not to expect more. It may even happen that those modern writers of our own whom we have agreed to call great Masters will have to take lower rank among posterity, who will have great Masters of their own. I am inclined, however, to think that a few of the nineteenth-century novelists will never be suffered to die, though they may be remembered principally for one book—that Thackeray will be remembered for his 'Vanity Fair,' Dickens for 'David Copperfield,' George Meredith for the 'Ordeal of Richard Feverel,' George Eliot for 'Silas Marner,' Charles Reade for the 'Cloister and the Hearth,' and Blackmore for his 'Lorna Doone.' On the other hand, without thinking or troubling ourselves at all about the verdict of posterity, which matters nothing to us compared with the verdict of our contemporaries, let us acknowledge that it is a bad year indeed when we have not produced some good work, work of a very high kind, if not immortal work. An exhibition of the year's novels would generally show two or three, at least, of which the country may be, say reasonably proud. Does the Royal Academy of Arts show every year more than two or three pictures—not immortal pictures, but pictures of which we may be reasonably proud? One would like, it is true, to see fewer bad novels published, as well as fewer bad pictures exhibited; the standard of the work which is on the borderland between success and failure should be higher. At the same time I am very sure and certain that there never has been a time when better works of Fiction have been produced, both by men and women. That Art is not declining, but is advancing, which is cultivated on true and not on false or conventional principles. Ought we not to be full of hope for the future, when such women as Mrs. Oliphant and Mrs. Thackeray Ritchie write for us—when such men as Meredith, Blackmore, Black, Payn, Wilkie Collins, and Hardy are still at their best, and such men as Louis Stevenson, Christie Murray, Clark Russell and Herman Merivale have just begun? I think the fiction, and, indeed, all the imaginary work of the future will be far fuller in human interest than in the past; the old stories—no doubt they will still be the old stories—will be fitted to actors who up to recently were only used for the purposes of contrast; the drama of life which formerly was assigned to kings and princes will be played by figures taken as much from the great struggling, unknown masses. Kings and great lords are chiefly picturesque and interesting on account of their beautiful costumes, and a traditional belief in their power. Costume is certainly not a strong point in the lower ranks, but I think we shall not miss that, and wherever we go for our material, whether to the higher or the lower ranks, we may be sure of finding everywhere love, sacrifice, and devotion for virtues, with selfishness, cunning, and treachery for vices, Out of these, with their endless combinations and changes, that novelist must be poor indeed who cannot make a story.

Lastly, I said at the outset that I would ask you to accord to novelists the recognition of their place as artists. But after what has been said, I feel that to urge this further would be only a repetition of what has gone before. Therefore, though not all who write novels can reach the first, or even the second, rank, wherever you find good and faithful work, with truth, sympathy, and clearness of purpose, I pray you to give the author of that work the praise as to an Artist—an Artist like the rest—the praise that you so readily accord to the earnest student of any other Art. As for the great Masters of the Art—Fielding, Scott, Dickens, Thackeray, Victor Hugo—I, for one, feel irritated when the critics begin to appraise, compare, and to estimate them: there is nothing, I think, that we can give them but admiration that is unspeakable, and gratitude that is silent. This silence proves more eloquently than any words how great, how beautiful an Art is that of Fiction.

[W. B.]

ANNUAL MEETING.

Thursday, May 1, 1884.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1883, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to above 85,400*l.*, entirely derived from the Contributions and Donations of the Members.

Thirty-seven new Members paid their Admission Fees in 1883.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1883.

The Books and Pamphlets presented in 1883 amounted to about 236 volumes, making, with 558 volumes (including Periodicals bound) purchased by the Managers, a total of 794 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, D.C.L. LL.D.
Treasurer—George Busk, Esq. F.R.S.
Secretary—Sir William Bowman, Bart. LL.D. F.R.S.

MANAGERS.

George Berkley, Esq. M.I.C.E.
Sir Frederick J. Bramwell, F.R.S.
Joseph Brown, Esq. Q.C.
Warren De La Rue, Esq. M.A. D.C.L. F.R.S.
Captain Douglas Galton, C.B. D.C.L. F.R.S.
Colonel James Augustus Grant, C.B. C.S.I. F.R.S.
The Hon. Sir Wm. Robt. Grove, M.A. D.C.L. LL.D. F.R.S.
Right Hon. The Lord Claud Hamilton, J.P.
Sir John Hawkshaw, F.R.S. F.G.S.
Sir John Lubbock, Bart. M.P. D.C.L. LL.D. F.R.S.
Hugo W. Müller, Esq. Ph.D. F.R.S.
Sir Frederick Pollock, Bart. M.A.
John Rae, M.D. LL.D. F.R.S.
The Earl of Rosse, D.C.L. LL.D. F.R.S.
The Hon. Rollo Russell, M.A. F.M.S.

VISITORS.

John Birkett, Esq. F.L.S. F.R.C.S.
Charles James Busk, Esq.
Stephen Busk, Esq.
George Frederick Chambers, Esq. F.R.A.S.
William Crookes, Esq. F.R.S.
Rear-Admiral Herbert P. De Kantzow, R.N.
William Henry Domville, Esq.
Alexander John Ellis, Esq. B.A. F.S.A. F.R.S.
Rev. John Macnaught, M.A.
Robt. Jas. Mann, M.D. F.R.C.S. F.R.A.S.
Sir Thomas Pycroft, M.A. K.C.S.I.
Lachlan Mackintosh Rate, Esq. M.A.
John Bell Sedgwick, Esq. F.R.G.S.
Basil Woodd Smith, Esq. F.R.A.S.
Charles Meymott Tidy, Esq. M.B. F.C.S.

WEEKLY EVENING MEETING,

Friday, May 2, 1884.

The Duke of Northumberland, D.C.L. LL.D. President, in the Chair.

Professor J. W. Judd, F.R.S. Sec. G.S.

Krakatoa.

The great subterranean convulsions which during the last few years have visited Croatia, Ischia, and Asia Minor, culminated in the autumn of 1883 in the grand volcanic outburst of Krakatoa, the most terrible and destructive event of its kind which has occurred within the memory of the present generation. It is not, however, true, as has been asserted, that this manifestation of volcanic energy was of altogether unparalleled magnitude or of unprecedented character; for within the last 120 years at least two paroxysmal volcanic outbursts on an equally grand scale have occurred in the same district—those namely of Papandayang in 1772, and of Tomboro in 1815.

Situated as it is in the middle of the Sunda Strait, one of the great highways of commerce, Krakatoa has been the subject of more exact observation—before, during, and since the eruption—than was possible in the case of any other volcano in equally violent activity. In spite of this, however, the first accounts brought to Europe concerning the great outburst were singularly inaccurate, and we are only now beginning to glean from the vast mass of conflicting reports the true story of the terrible event. Certain it was, however, that on the 26th and 27th of August, 1883, the shores of Java and Sumatra were swept by a great sea-wave which desolated considerable tracts of country and destroyed the lives of more than 35,000 human beings, and that this sea-wave was one of the most striking accompaniments of a paroxysmal outburst of Krakatoa.

The volcano of Krakatoa lies at the intersection of two great fissures—indicated by numerous volcanic vents—in that part of the earth's crust where we have the most abundant indication of subterranean energy. The group of four small islands in the midst of the Sunda Strait was evidently the "basal-wreck" of a grand volcanic cone, which had been destroyed by a paroxysmal outburst in prehistoric times. It is not improbable that the subsidence accompanying this great outburst gave rise to the depression which forms the strait now separating Java and Sumatra. About 200 years ago the volcano is known to have been in eruption for a period of eighteen months, but since that time it has remained perfectly dormant.

During the last few years numerous earthquakes have indicated that the district of which Krakatoa is the centre was likely to again become the scene of volcanic disturbance, and on the 20th of May, 1883, Krakatoa again burst into activity. This eruption, which was of moderate violence, continued for about three months, resulting in the formation of two cinder-cones of considerable size, and the scattering of large quantities of pumice over the surface of the ocean. The materials of Krakatoa and the neighbouring volcanoes are almost entirely pumiceous in character, being formed through the distension by gases of a hypersthene-augite-andesite with an exceptionally large proportion of a highly vitreous base.

On the afternoon of August 26th, the volcano passed from the stage of continued moderate activity, to a paroxysm of great violence. During this paroxysm, the whole district for 100 miles around the volcano was enveloped in intense darkness, produced by the enormous clouds of volcanic dust thrown into the atmosphere, some of which fell at distances of over 1000 miles from Krakatoa. Our knowledge of what took place during this terrible outbreak is derived from the reports of the few survivors in the towns on the shores of the strait, from the logs of various ships, three of which were actually within the strait at the time of the eruption, from the indication afforded by self-recording instruments at Batavia and more distant localities, and by a comparison of the condition of the volcano before and after the eruption.

It appears that during the height of the eruption the detonations of the volcano increased in number and violence till they blended in a continuous roar, and that enormous quantities of steam with pumice and dust were flung to very great heights in the atmosphere. The result of this action was the blowing away, not only the two cones recently formed, but of the greater part of the old volcano, leaving a vast crateral hollow more than 1000 feet in depth. As to the earthquake shocks accompanying this paroxysm, the accounts are very conflicting. There was unfortunately no seismograph at Batavia, but the magnetograph-records seem to indicate that considerable seismic disturbance took place during the whole time of the eruption. The precisely similar instrument at Kew Observatory recorded in the same way the small earthquake of April 22nd, 1884.

No doubt exists, however, as to the nature and magnitude of the sea-waves produced by these great concussions. During the whole time of the eruption, the ocean was thrown into a state of violent oscillations, the oscillations increasing in height as the eruptive action became more intense. In the narrow and shallow eastern throat of the Sunda Strait, and in the deep gulfs of Lampong, Semangka, and Welcome Bay, these vibratory movements of the water were converted into waves of translation of great height and destructiveness. Westward, however, they were propagated across the Indian Ocean, like the ordinary oceanic tidal wave, at rates varying from 350 to 500 miles per hour, recording themselves on the tide-gauges all over the world. At Mauritius and Port Elizabeth, at Aden and the principal Indian ports, and even as far away as Australia, New Zealand, San

Francisco and Alaska, these disturbances of the ocean were felt and recorded.

Still more striking were the vibrations propagated through the more mobile material of the atmosphere. The effects produced on the atmosphere in the vicinity of Krakatoa by the violently up-rushing columns of vapour, and by its condensation, were indicated by frequent and sudden changes in the height of the barometric column, and by a terrible storm, of a strictly local character, which raged during the whole time of the eruption. The investigations of Mr. Scott and General Strachey have demonstrated that these disturbances were propagated in a series of waves, which, travelling at the rate of 700 miles per hour, passed three-and-a-half times round the globe, recording themselves on the barographs of meteorological stations all over the world.

The vibrations producing sound, whether carried by the land, the ocean, or the air, made themselves felt over a circle with a radius of 2000 miles.

The electrical disturbances, resulting in vivid lightning "fire-balls," corposants, and a phosphorescent condition of the ejected materials, were of the most startling character.

The careful surveys undertaken since the eruption by the officers of the Dutch Government, have shown that the whole of the island of Krakatoa, except the high ridge on its southern side, was blown away during the eruption, but that two adjoining smaller islands were increased in size. To the north and north-east of Krakatoa, at a distance of 7 miles from the centre of activity, two new islands were formed where the sea had before a depth of about 20 fathoms. As these new islands are entirely composed of loose fragmentary materials, it has been thought that they were formed by the accumulation of the ejecta from the central vent. There are strong grounds however for the belief that lateral eruptions causing submarine volcanoes accompanied the outburst from the central crater of Krakatoa. The quantity of pumice thrown out during the eruption was so great as to impede the navigation of the strait, and to cover the ocean for thousands of square miles.

Comparing the outburst of Krakatoa with that of Tomboro in 1815, we find that the quantity of material ejected in the latter was, according to Verbeek, from eight to eleven times as great as in that of the former, while the area of darkness produced by the dust was no less than nine times as great. On the other hand, it must be remembered that the duration of the Tomboro eruption was more than thirty days, while that of the Krakatoa was less than that number of hours. Hence we are led to conclude that the Krakatoa eruption, though of short duration, was of exceptional violence, a conclusion borne out by the fact that it was heard at distances twice as great as the outburst of Tomboro.

Of the interesting atmospheric effects, and especially the beautiful sunsets, following the Krakatoa eruption, which have with a great show of probability been thought to have resulted from it, it is the province of the physicist and meteorologist, rather than of the geologist, to speak. Without attempting to discuss any of the different explanations that have been offered to account for these strikingly beautiful phenomena, it is only just to remark that the basis of all such explanations will probably be found in the experiments carried on by Faraday at the Royal Institution, which demonstrated the excessive divisibility of matter and the effect of finely divided particles on light, with others, subsequently made by Professor Tyndall, which suggested the application of these principles to the explanation of the colours of the atmosphere.

[J. W. J.]

GENERAL MONTHLY MEETING,

Monday, May 5, 1884.

Warren De La Rue, Esq. M.A. D.C.L. F.R.S. Manager,
in the Chair.

Rookes Evelyn Crompton, Esq.
Ernest George Mocatta, Esq.
Ernest Robert Moon, Esq.
John Lawrence Tatham, Esq.

were elected Members of the Royal Institution.

John Tyndall, Esq. D.C.L. LL.D. F.R.S. was re-elected Professor of Natural Philosophy.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Memoirs, Vol. XX. Parts 1 and 2. 8vo. 1883.
The Secretary of State for India—Report on Public Instruction in Bengal, 1882–3. fol. 1883.
The Trustees of the British Museum—Catalogue of Birds, Vol. IX. 8vo. 1884.
Academy of Natural Sciences, Philadelphia—Proceedings, 1883, Part 3. 8vo. 1883.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Transunti. Vol. VIII. Fasc. 7–10. 4to. 1884.
Agricultural Society of England, Royal—Journal, Second Series, Vol. XX. Part 1. 8vo. 1884.
Asiatic Society, Royal—Journal, Vol. XVI. Part 2. 8vo. 1884.
Bankers, Institute of—Journal, Vol. V. Part 4. 8vo. 1884.

Birmingham Philosophical Society—Proceedings, Vol. III. Parts 1 and 2. 8vo. 1881-3.
British Architects, Royal Institute of—Proceedings, 1883-4, No. 11. 4to.
Browne, Lennox, Esq. (*the Author*)—Science and Singing. 8vo. 1884.
Burt, Major Thomas Seymour, F.R.S. (*the Translator*)—The Æneid, Georgics, and Eclogues of Virgil. In English Blank Verse, with Latin Text. 3 vols. 8vo. 1883.
Chemical Society—Journal for April 1884. 8vo.
Civil Engineers' Institution—Minutes of Proceedings, Vol. LXXV. 8vo. 1884.
Applications of Electricity, 1882-3. 8vo. 1884.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (*the Editor*)—Journal of the Royal Microscopical Society, Series II. Vol. IV. Part 2. 8vo. 1884.
Dax: Société de Borda—Bulletins, 2^{e} Serie Neuvième Année: Trimestre 1. 8vo. 1884.
East India Association—Journal, Vol. XVI. No. 2. 8vo. 1884.
Editors—American Journal of Science for April 1884. 8vo.
Analyst for April 1884. 8vo.
Athenæum for April 1884. 4to.
Chemical News for April 1884. 4to.
Engineer for April 1884. fol.
Horological Journal for April 1884. 8vo.
Iron for April 1884. 4to.
Nature for April 1884. 4to.
Revue Scientifique and Revue Politique et Littéraire for April 1884. 4to.
Science Monthly, Illustrated, for April 1884. 8vo.
Telegraphic Journal for April 1884. 8vo.
Franklin Institute—Journal, No. 700. 8vo. 1884.
Geological Institute, Imperial, Vienna—Jahrbuch, Band XXXIII. No. 4. 8vo. 1883.
Grey, Henry, Esq. (*the Author*)—A Bird's Eye View of English Literature. 12mo. 1884.
Hanks, Henry G. Esq. (*State Mineralogist*)—California State Mining Bureau, Annual Report. 8vo. 1883.
Hungarian Academy of Sciences—Mathematische und Naturwissenschaftliche Berichte aus Ungarn. Band I. 8vo. 1883.
Inwards, Richard, Esq. F.R.A.S. (*the Author*)—The Temple of the Andes. 4to. 1884.
Iron and Steel Institute—Journal, 1883, No. 2. 8vo.
Johns Hopkins University—University Circulars, No. 29. 4to. 1884.
Marks, W. Dennis, Esq. (*the Author*)—The Law of Condensation of Steam in Cylinders. (Jour. of Franklin Inst. 1884.) 8vo.
Medical and Chirurgical Society—Proceedings, New Series, No. 5. 8vo. 1884.
Meteorological Society, Royal—Quarterly Journal, No. 49. 8vo. 1884.
Meteorological Record, No. 11. 8vo. 1883.
National Association for Social Science—Transactions, Huddersfield, 1883. 8vo. 1884.
Proceedings, Vol. XVII. No. 1. 8vo. 1884.
Pharmaceutical Society of Great Britain—Journal, April 1884. 8vo.
Photographic Society—Journal, New Series, Vol. VIII. No. 6. 8vo. 1884.
Richardson, B. W. M.D. F.R.S. (*the Author*)—The Asclepiad. Vol. I. No. 2. 8vo. 1884.
Rio de Janeiro, Observatoire Imperiale—Bulletin, Nos. 3, 11. fol. 1881-3.
Society of Arts—Journal, April 1884. 8vo.
Statistical Society—Journal, Vol. XLVII. Part 1. 8vo. 1884.
Telegraph Engineers, Society of—Journal, Vol. XIII. No. 51. 8vo. 1884.
University of London—Calendar, 1884-5. 8vo.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1884: Heft 3. 4to.
Victoria Institute—Journal, No. 68. 8vo. 1884.

Westropp, Hodder M. Esq. (the Author)—The Age of Homer. 8vo. 1884.
Williams, Charles J. B. M.D. F.R.S. (the Author)—Memoirs of Life and Work. 8vo. 1884.
Williamson, B. Esq. M.A. F.R.S. (the Author)—Elementary Treatise on the Integral Calculus. 4th ed. 8vo. 1884.
Zoological Society—Proceedings, 1883, Part 4. 8vo.
Catalogue of the Library, Supplement. 8vo. 1883.

WEEKLY EVENING MEETING,

Friday, May 9, 1884.

The Duke of Northumberland, D.C.L. LL.D. President, in the Chair.

Professor W. Robertson Smith, M.A. LL.D.

Mohammedan Mahdis.

(Abstract deferred.)

WEEKLY EVENING MEETING,

Friday, May 16, 1884.

Sir Frederick Bramwell, F.R.S. Manager and Vice-President, in the Chair.

Professor W. Odling, M.A. F.R.S. *M.R.I.*

The Dissolved Oxygen of Water.

(Abstract deferred.)

WEEKLY EVENING MEETING,

Friday, May 23, 1884.

The Earl of Rosse, D.C.L. LL.D. F.R.S. Manager and Vice-President, in the Chair.

David Gill, Esq. LL.D. F.R.S.
Her Majesty's Astronomer at the Cape of Good Hope.

Recent Researches on the Distances of the Fixed Stars, and some Future Problems in Sidereal Astronomy.

There has ever been a desire to burst aside the constraints imposed upon our research by the distances of space; to pass from the study of the planets of our solar system to that of the suns and galaxies that surround us; to determine the position and relative importance of our own system in the scheme of the universe, and the whence we have come and the whither we are drifting through the realms of space.

The galaxy or Milky Way—what is it? Is our sun one of its members? What is the shape of that galaxy? What are its dimensions? What is the position of our sun in it?

The star-clusters—what are they? Are these clusters galaxies? Have these suns real dimensions comparable with those of our sun, and is it distance alone that renders their light and dimension so insignificant to the naked eye? Or are the real dimensions of the clusters small as compared with our galaxy? Are their component suns but the fragments of some great sun that has been shattered by forces unknown to us, or have they originated from chaotic matter, which, instead of forming one great whirlpool and condensing by vortex action into one great sun, has been thrown into numerous minor vortices, and so become rolled up into numerous small suns?

The nebulæ—what are they? Are they, too, condensing into clusters or stars, or will their ghost-like forms remain for ever unchanged amongst the stars? or do they play some part in the scheme of nature of which we have as yet no conception?

These and many others are the questions which press on the ardent mind that contemplates the subject; and there arises the intense desire to answer such questions, and, where facts are wanting, to supply facts by fancy. The history of deep and profound thought in some of these subjects goes back through 2000 years, but the history of real progress is but as of yesterday. The foundation of sidereal astronomy may be said to have begun with the art of accurate observation. Bradley's meridian observations at Greenwich about 1750, his previous discovery of the aberration of light in 1727, and Herschel's discovery of the binary nature of double stars, his surveys of the heavens, and his catalogues of double stars—these are solid facts, facts that have contributed more to the advancement of sidereal astronomy than all the speculations of preceding centuries. They point to us the lesson that "art is long and life is short," that human knowledge, in the slow developing phenomena of sidereal astronomy, must be content to progress by the accumulating labours of successive generations of men, that progress will be measured for generations yet to come more by the amount of honest, well-directed and systematically-discussed observation than by the most brilliant speculation, and that in observation concentrated systematic effort on a special thoughtfully-selected problem will be of more avail than the most brilliant but disconnected work.

I hope that no one present thinks from what I have said that I undervalue the imaginative fervid mind that longs for the truth, and whose fancy delights to speculate on these great subjects. On the contrary, I think and I believe that without that fervid mind, without that longing for the truth, no man is fitted for the work required of him in such a field—for it is such a mind and such desires that alone can sweeten the long watches of the night, and transform such work from drudgery into a noble labour of love.

It is for like reasons that I ask you to leave with me the captivating realms of fancy this evening, and to enter the more substantial realms of fact.

We suppose ourselves, then, face to face with all the problems of sidereal astronomy to which I have hastily referred—the human mind is lost in speculation, and we are anxious to establish a solid groundwork of fact.

Now what in such circumstances would be the instinct of the scientific mind?

The answer is unquestionable—viz. to measure—and no sooner were astronomical instruments made of reasonable exactness than astronomers did begin to measure, and to ask, are the distances of the fixed stars measurable?

I should like to have given a short history of the early attempts of astronomers to measure the distance of a fixed star. But I must come at once to the time when the long baffled labours of astronomers began to be crowned with success.

Before I begin, it will save both time and circumlocution if I define a word that we must frequently use—viz. the word "parallax."

It may be defined as the change in the apparent place of a star produced by viewing it from a point other than that of reference. [The lecturer here gave some practical illustrations of parallax.] Our point of reference for stars is the sun, and as we view the stars now from one side of the sun, and six months afterwards from a point on the opposite side of the sun—that is, from two points 186 millions of miles apart—we might expect to find a considerable change in their apparent places.

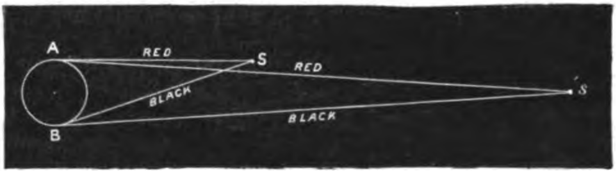
But previous to 1832 astronomers could not discover with any certainty that such changes were sensible—or, putting it another way, the stars were so distant that the diameter of the earth's orbit viewed from the nearest star subtended a smaller angle than their instruments could measure. Bradley felt sure that if the star γ Draconis were so near that its parallax amounted to 1″ of arc he would have detected it—that is, if the earth's orbit viewed from γ Draconis measured 2″ in diameter (or as big as a globe 1 foot in diameter would look if viewed at 40 miles distant) he would have detected it. But the real distances of the stars were greater than that.

The time at last arrived when the two great masters of modern practical astronomy, Bessel and Struve, were preparing by elaborate experiment and study for the researches which led to ultimate success. After vain attempts to obtain conclusive results by endeavours to determine the apparent changes in the absolute direction of a star at different seasons of the year, both astronomers had recourse to a method which, originally proposed by Galileo in 1632, was carried out first on a large scale by Sir William Herschel. I shall refer in the first place to the researches of the great Russian astronomer Struve.

Astronomers had sufficiently demonstrated that the distances of the stars were very great, and it was reasonable to argue that as a rule the brighter stars would be those nearest to us. If, therefore, two stars are apparently near each other—the one bright, the other faint—the chances are that in reality they are far apart, though accidentally nearly in a line.

If two such stars are represented by S *s* in Diagram I., they would

Diagram I.



appear near each other viewed from one side of the earth's orbit at A, but not so near each other viewed from B—the opposite side of the earth's orbit, the red lines obviously indicating the apparent angle between the stars when they are viewed from A, and the black lines the apparent angle when they are viewed from B. Struve selected for the star S the bright star Vega (α Lyræ). From its brilliancy he considered it probably one of our nearest neighbours amongst the stars, and a faint star apparently near it seemed to afford a suitable representative of the really distant star *s*. Struve was careful to ascertain that this comparison star was not physically connected with α Lyræ, and he was able to prove this from the fact that whilst α Lyræ has a small annual motion relative to all neighbouring stars, this motion is not shared by the faint comparison star. Struve was provided with a telescope driven by clockwork to follow the diurnal motion of a star, and thus the hands of the observer were free to make the necessary measures. These were accomplished by an instrument, such as I hold in my hands, applied to the telescope. This micrometer contains two parallel spider-webs each attached to a slide, one slide being moved by one screw, the other by the other screw. The screws are provided with drum-heads divided into 100 parts. One web was placed on the image of α Lyræ, the other upon that of the faint comparison star, and the angle between the stars was thus read off in terms of the number of revolutions and decimals of a revolution of the screws. A number of such observations was made on each night, and the result for each night depended on the mean of the numerous observations made each night.

By observations on ninety-six nights between November 1835 and August 1838, he showed that the distance between α Lyræ and the faint comparison star changed systematically with a regular annual period, and that the maxima and minima of those distances corresponded with the times of the year at which these maxima and minima should occur if the brighter star were really much nearer than the fainter one.

Assuming that the fainter star is at a practically immeasurable distance, Struve showed that α Lyræ had a parallax that amounted to about a quarter of a second of arc, which is equivalent to the statement that a globe whose diameter is equal to that of the earth's orbit —that is, to 186 millions of miles—would at the distance of α Lyræ present an apparent diameter of half a second of arc. If you wish to realise this angle, place a globe 1 foot in diameter at a distance of 80 miles, or look at a coin half the diameter of a silver threepenny-piece at a distance of 1 mile from the eye, and try to measure it.

The great German astronomer, Bessel, was simultaneously engaged in like work at Königsberg. He selected as the object of his researches a very remarkable double star—61 Cygni.

This star had already been the subject of similar researches on his part with much inferior means. He now attacked the problem with the splendid heliometer which had been made for him by Frauenhofer for the purpose. The principle of this instrument I shall presently explain. His reasons for choosing 61 Cygni were that the two components of this star, though not remarkable for brightness—they are just visible to the naked eye—yet have this peculiarity, that they have a remarkably large proper motion, the largest then known, though now known to be surpassed by that of two other stars which I shall afterwards mention. The components of 61 Cygni have an apparent angular motion relative to other stars of more than five seconds of arc per annum.

Struve had argued that if the stars were on the average of similar brightness, those stars which were brightest would probably be those nearest to us, and Bessel, in like manner, argued that if the absolute motions of the stars were similar on the average, those motions which appeared the largest belonged to stars which on the average were nearest to us—just as the motion of a snail could be easily watched at the distance of two or three feet from the eye, but could not be detected except after a long interval if the animal were a good many yards distant.

Bessel employed two faint comparison stars at right angles to each other with respect to 61 Cygni, and he made two separate series of observations, the first extending from August 1837 to October 1838, the second from October 1838 to March 1840.

Both series confirm each other, and the results deduced separately from the measures of the two comparison stars also agree within very narrow limits. From all the observations combined Bessel found the parallax of 61 Cygni to be 35/100 of a second—a quantity which has been shown by the modern researches of Prof. Auwers and Dr. Ball to be more nearly half a second of arc. Thus at 61 Cygni the diameter of the earth's orbit round the sun would appear of the same size as a globe a foot in diameter viewed at 40 miles distance, or of a silver threepenny-piece a mile off. But whilst these great masters of astronomy—Struve and Bessel—had been exhausting the resources of their skill in observation, and that of the astronomical workshops of Europe in supplying them with the most refined instruments, a quiet and earnest man had been at work at the Cape of Good Hope, and, without knowing it at the time, had *really made the* FIRST *observations* which afforded strong presumptive evidence of the existence of the parallax of any fixed star.

Henderson occupied the post of Her Majesty's Astronomer at the Cape of Good Hope in 1832 and 1833, and during his brief and brilliant tenure of office there, he made, amongst many others, a fine series of meridian observations of α Centauri—a bright and otherwise remarkable double star. When, after his return to England, Henderson reduced these observations, and compared them with the earlier observations of other astronomers, he found that α Centauri had a large proper motion; he was therefore led to examine and see whether his observations gave any indication of an annual parallax. He found that they did so, and not of a small parallax but of one amounting to nearly a second of arc. But it was not till this was confirmed, not only by the observations with the mural circle but by those of the transit instrument also, not only by his own observations but by those of Lieut. Meadows, his assistant, that Henderson ventured to publish his remarkable result.

In the year 1842 it was felt by the astronomical world at large that the problem which hitherto had baffled astronomers had begun to yield, that some approximation to the truth had at last been arrived at with regard to the distance of a fixed star, and it was fit and proper that the Royal Astronomical Society of London should acknowledge the labours of him who had most effectually contributed to this end.

Henderson's results seemed sufficiently convincing, but they depended upon determinations of the absolute place of α Centauri. The experiences of the skilful astronomer Brinkley at Dublin were still fresh in the minds of astronomers. He had arrived by similar, though less perfect, means at results like those of Henderson; but his results had been proved to be fallacious, though the causes of their being so still remain somewhat inexplicable. In the case of Struve's observations the weight of evidence which he produced and the excellence of his method were admitted, but men were not prepared by experience for accepting as accurate the minute changes of angle which Struve had to measure; nor was the proof afforded by his series of observations so entirely convincing as that afforded by the series of Bessel. Therefore, to Bessel the well-earned medal was given, but the labours of Struve and Henderson received high and honourable mention. I quote from the speech of Sir John Herschel in awarding that medal. He says of Henderson's researches on *α* Centauri:—

"Should a different eye, and a different circle continue to give the same result, we must of course acquiesce in the conclusion; and the distinct and entire merit of the *first* discovery of the parallax of *a* fixed star will rest indisputably with Mr. Henderson. At present, however, we should not be justified in anticipating a decision which time alone can stamp with the seal of absolute authority."

So much for Sir John Herschel's officially expressed opinion. I can state now, and as Henderson's successor I do so with pride and pleasure, that a different eye (that of his able and sympathetic successor, Sir Thomas Maclear) fully confirmed Henderson's result with another circle; and further, that Henderson's result has been still further confirmed by additional researches of which I shall presently speak.

I must now pass over briefly the history of succeeding researches, and indeed it has been so admirably and so recently told within these walls by Dr. Ball, that it is quite unnecessary I should enter upon it in detail. The most reliable values arrived at for the parallaxes of the stars of the northern hemisphere are given in the following table, and to these results I shall afterwards refer:—

TABLE I.—PARALLAXES OF STARS WHICH HAVE BEEN DETERMINED IN THE NORTHERN HEAVENS WITH CONSIDERABLE ACCURACY.

	Magnitude.	Proper Motion.	Parallax.
		"	"
61 Cygni	6	5·14	0·50
Lalande 21185	7¼	4·75	0·50
α Tauri	1	0·19	0·52
34 Groombridge	8	2·81	0·29
Lalande 21258	8½	4·40	0·26
O. Mg. 17415	9	1·27	0·25
σ Draconis	5½	1·87	0·25
α Lyræ	1	0·31	0·20
p Ophiuchi	4½	1·0	0·17
α Bootis	1	2·43	0·13?
Groombridge 1830	7	7·05	0·09
Bradley 3077	6	2·09	0·07
85 Pegasi	6	1·38	0·05

The recent researches referred to in the title of this evening's lecture are some investigations which, in conjunction with a young American friend, Dr. Elkin, who was my guest for two years, I have recently carried out at the Cape of Good Hope.

The instrument employed was a heliometer—my own property—the good qualities of which I had previously tested at Mauritius in 1874, and at the Island of Ascension in 1877.

[The lecturer here described the heliometer, and illustrated the method of its use.]

I have said that the angle between the stars is measured in terms of the scale of the heliometer, but the scale-value, in seconds of arc, may change by the effects of temperature and from other causes.

Bessel in his researches on the parallax of 61 Cygni, determined by independent means the effect of temperature on his scale-value, and applied corresponding corrections to his observations. But he also took the precaution to employ two stars of comparison situated at right angles to each other with respect to the principal star, so that the effect of parallax would be at a maximum for one comparison star at the season of the year when it was at zero for the other, and *vice versâ.*

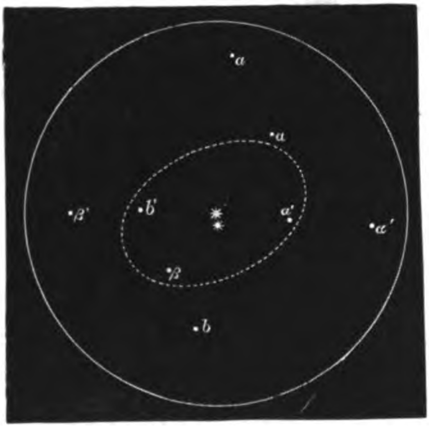
But in the course of previous researches I found that there were sources of error other than mere change of the temperature of the air, viz. differences of temperature in different parts of the instrument, and changes in the normal focus of the observer's eye, which exercised a very sensible influence on the results. It was necessary to devise some method by which these should also be eliminated.

There is a very simple means of doing this. Instead of taking two comparison stars at right angles, take two comparison stars situated nearly symmetrically on opposite sides of the star whose parallax is to be determined—such, for example, as the stars α and β in Diagram II. Now observe these distances in the order α, β, β, α, on each night of observation; so that on each night the observations at both distances are practically made at the same instant. Then, whatever causes have combined to create a systematic error in the measurement of one of these distances, precisely the same causes must create precisely similar systematic error in the measurement of the other distance. Thus if, by the regular or irregular effects of temperature or by changes in the normal condition of the observer's eye, we measure the distance α too great, so for the simultaneous observations of the distance β we shall, from precisely the same causes, measure that distance too great also.

But the *difference* of the distances will be entirely free from all errors of the kind; and if the distances are not quite equal, it is very easy to apply a correction on the assumption that the sum of the distances is a constant.

In Diagram II. the circle represents a radius of 2° surrounding the star α Centauri. The distance of the component stars α_1 and α_2 Centauri in the diagram is enormously exaggerated for the sake of clearness. Guided by the principles just explained, search was made for comparison stars in pairs symmetrically situated with respect to *α* Centauri, and otherwise favourably situated for measurement of parallax.

DIAGRAM II.



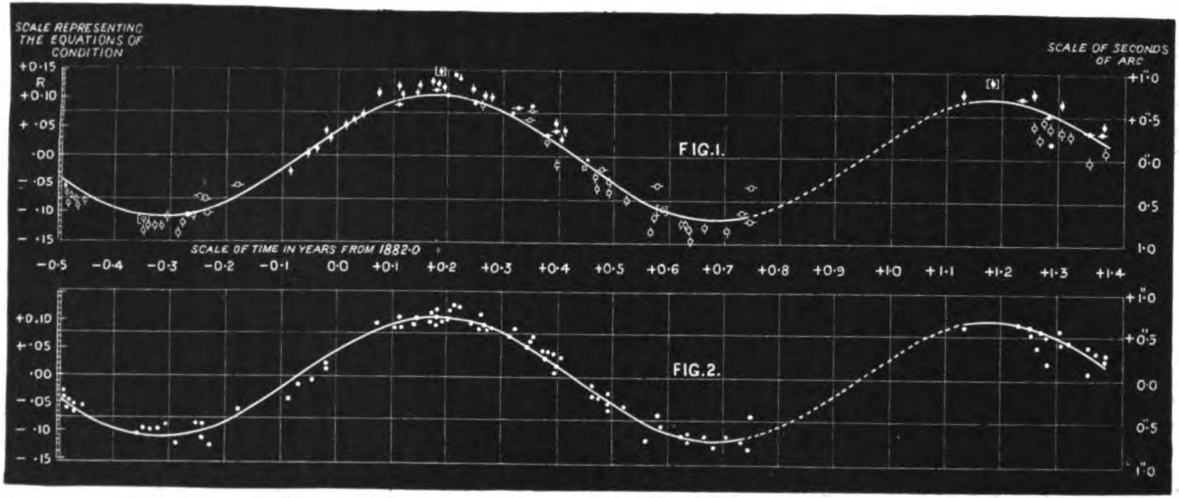
Showing comparison stars employed in determining the parallax of *α* Centauri.

You will remember that from the effects of parallax all stars appear to describe small ellipses about a mean position; stars near the pole of the ecliptic describing nearly circles, and those near the ecliptic very elongated ellipses. Obviously, then, those pairs of stars are most favourable—other conditions being equal—which lie near the major axis of the parallactic ellipse. The dotted ellipse in Diagram II. represents the *form* of the parallactic ellipse; that is to say, the form of the apparent path which *α* Centauri must describe if it is affected by parallax. Of course the *size* of the ellipse is exaggerated—in fact in the diagram nearly 5000 times—therefore, remember that the diagram represents only that which we can compute before we have observed, viz. the *shape* of the ellipse, or the relations of the lengths of the two axes; the *absolute* size has to be determined from the observations.

The most favourable couple of comparison stars in our drawing is that marked *α* and *β*—they are nearest to the major axis of the parallactic ellipse, and they are very symmetrically situated with respect to *α* Centauri.

Now turn to Diagram III. Here is exhibited the results of my measures on a very large scale—in a manner similar to that in which the height of the barometer for different hours of the day, or the comparative price of wheat at different seasons of the year or in different

Diagram III.



Curves showing the results of the observations of *α* Centauri relative to the comparison stars *α* and *β*.

years, is now exhibited in the daily papers. Imagine the star α about a mile immediately below any point of that curve, and the star β rather over three-quarters of a mile immediately above the same point, and you would then have a diagram to scale.* The middle horizontal line represents the mean difference of these two distances, and each dot or mark on Fig. 1 of the diagram represents the variation of that distance according to each successive observation. The different kinds of dot represent measures made at different hour angles, or when the relation of the direction of measurement to the line joining the observer's eye is different. These different kinds of personal errors were separately investigated, and they were then allowed for and the observations were corrected accordingly.

The observations so corrected are represented in Fig. 2, where each black dot expresses the result of the observations of a single night, and the curve is the commuted curve resulting from the mathematical discussion of the observations.

You must be careful to understand that this is not simply the kind of curve which best represents the observations. The curve is limited by purely geometrical conditions to have its maximum on March 7, and its minimum on September 10, and to follow a precise form of curve according to a simple law. The observations only determine the range from maximum to minimum, and yet you see how perfectly the maximum of the observations agrees with the maximum of the curve, and the minimum of the observations with the minimum of the curve, and how closely the law is followed throughout.

The result was that from these observations the parallax of α Centauri was $0''\cdot747$, or practically three-quarters of a second of arc.

But I was not content with this result alone. I wished further confirmation, and selected another pair of stars, α' and β', shown in Diagram II.

From similar observations with these comparison stars I obtained for the parallax of α Centauri $0''\cdot760$, a result which is identical with the last within the limits of the probable error of either.

My friend Dr. Elkin selected the stars *a b* and *a′ b′* as his comparison stars, and in a precisely similar way he obtained as the mean of his results a parallax of $0''\cdot752$, a result identical with my own, so that we may conclude as one of the most certainly established facts of astronomy that the parallax of α Centauri relative to an average star of the seventh or eighth magnitude is three-quarters of a second of arc.

It is therefore beyond all doubt that Henderson's discovery was a real one. Herschel's verdict must therefore be confirmed, and the palm for first breaking down the barriers that separated us from any knowledge of the distances of the fixed stars be accorded to the memory of the Cape Astronomer, Henderson.

---

* In the wall diagram one second of arc was represented by about fifteen inches.

So far as all existing researches go, *α* Centauri is the nearest of the fixed stars. Regarding the faint comparison stars as practically infinitely distant, let us try to realise how near or how far distant *α* Centauri really is.

If we wish to deal with distance so immense, we must adopt a more convenient unit of measure.

The most convenient unit for our purpose is the number of years that light would take to reach us. Light takes almost exactly 500 seconds of time to come from the sun; this is a figure easy to remember, and is probably exact to a single unit. The sun is 93 millions of miles distant, and this figure I believe to be exact within 200,000 miles.

Quite recently the accuracy of these figures has been confirmed in a very remarkable way by different kinds of investigations by different observers; otherwise I should not have quoted them with so much confidence.

The parallax of *α* Centauri is three-quarters of a second of arc; therefore its distance is 275,000 times the distance of the earth from the sun, and therefore light, which travels to the earth from the sun in 500 seconds (i. e. in 8⅓ minutes) would take 4·36, or a little more than 4⅓ years to come from *α* Centauri.

You will find in the accompanying table a specific account of the other results which were arrived at by Dr. Elkin and myself by precisely similar means, and you will find on the wall diagrams representing my own detailed observations in the case of Sirius and *ε* Indi. (See Diagrams IV. and V.)

TABLE II.—RESULTS OF RECENT RESEARCHES ON THE PARALLAX OF STARS IN THE SOUTHERN HEMISPHERE.

Name of Star.	Observer.	Star's magnitude.	Annual proper motion in arc.	Parallax.	Star's distance in light units, or number of years in which light from star would reach the earth.	Velocity of star's motion in miles per second at right angles to line of sight.
α Centauri ..	G. & E.	1	3″·67	0″·75	4·36	14·4
Sirius ..	G. & E.	1	1·24	0·38	8·6	9·6
Lacaille 9352	G.	7½	6·95	0·28	11·6	73
ε Indi ..	G. & E.	5¼	4·68	0·22	15	63
o$_2$ Eridani ..	G.	4½	4·10	0·17	19	69
e Eridani ..	E.	4½	3·03	0·14	23	64
ζ Tucanæ ..	E.	6	2·05	0·06	54	101
Canopus ..	E.	1	0·00	Insensible	—	—
β Centauri	G.	1	—	Insensible	—	—

Time does not permit me to go into more detail as to each of these separate results, full of interest though they are, and each of them representing months of labour.

My object now is to generalise, to put out the conclusions that must be drawn from these two tables of parallax (Tables I. and II.), and to see what are the broad lessons that they teach us.

A glance is sufficient to show that neither apparent magnitude nor apparent proper motion can afford a definite criterion of the distance of any fixed star—that different stars really differ greatly in absolute brightness and in absolute motion.

And now what is the work before us in the future?

The great cosmical problem that we have to solve is not so much what is the parallax of this or that particular star, but we have to solve the much broader questions—

1. What are the average parallaxes of stars of the *first*, *second*, *third*, and *fourth* magnitudes, compared with those of fainter magnitude?

2. What connection does there subsist between the parallax of a star and the amount and direction of its proper motion, or can it be proved that there is no such relation or connection?

With any approximate answer to these questions we should probably be able to determine the law of absorption of star-light in space, and be provided with the data at present wanting for determining with more precision the constant of precession and the amount and direction of the solar motion in space. And who can predict what hitherto unknown cosmical laws might reveal themselves in the course of such an investigation?

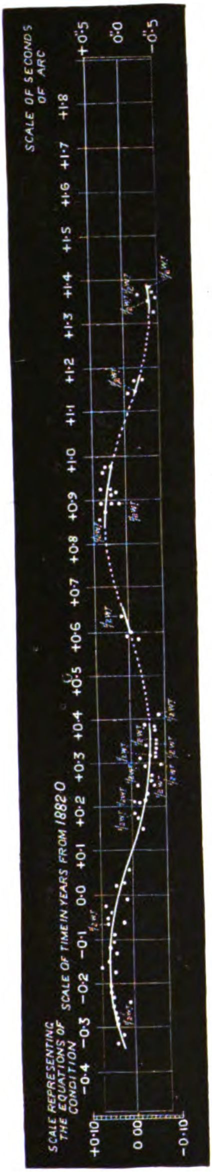
It is important to consider whether such a scheme of research is one that can be realised in the immediate future, or one that can only be carried to completion by the accumulated labours of successive astronomers.

I have very carefully considered this question from a practical point of view, and I have prepared a scheme, founded on the results of my past experience. I have submitted that scheme for the opinion of the most competent judges, and in their opinion, as well as my own, the work can be done, with honest hard work for one hemisphere, within ten years. I have offered to do that work for the southern hemisphere with my own hands, and a proposal for the necessary instruments and appliances is now under the consideration of my Lords Commissioners of the Admiralty. I need hardly add that in this matter I look confidently for that complete consideration and that efficient support which I have never failed to receive at their hands since I have had the honour to serve them.

The like work will be undertaken for the northern hemisphere by my friend Dr. Elkin, who is now in charge of the heliometer at Yale College in America. It is at present the finest instrument of the kind in the world, and a photograph of it you have already seen upon the screen.

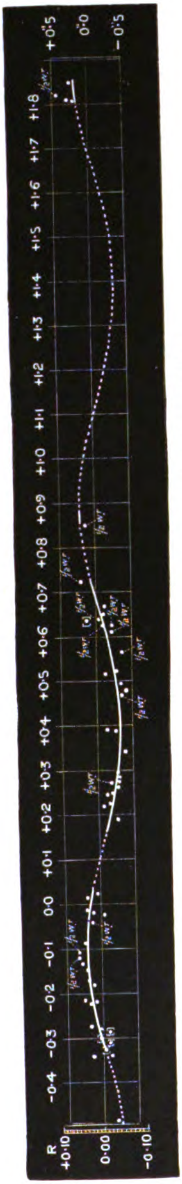
I most earnestly trust that we may be granted health and strength for this work, and that no unforeseen circumstances will prevent its complete accomplishment.

Diagram IV.



Curve showing results of observations of Sirius for parallax.

Diagram V.



Curve showing results of observations of ε Indi for parallax.

Before closing this lecture I wish briefly to allude to another engine of research in sidereal astronomy which quite recently has received an enormous development, and whose application appears to offer a rich harvest of results. I refer to the application of photography to astronomical observation.

Your respected member, Mr. De la Rue, is the father of this method. Time does not permit me to dwell on his early endeavours and his successful results, but they are well known to you all. He opened up the field, and he cleared the way for his successors.

The recent strides in the chemistry of photography and the production of dry plates of extreme sensibility, have permitted the application of the method to objects that formerly could not be photographed. Here, on the screen, are the spectra of stars photographed directly from the stars by Dr. Huggins, the lines which tell of the chemical constitution and temperature of the star's atmosphere being sharply defined.

Here are photographs of the great comet of 1882, which, with the co-operation of Mr. Allis of Mowbray, I obtained at the Cape, by attaching his ordinary camera to an equatorially mounted telescope, and with its aid following the comet exactly for more than two hours. Each one of the thousands of points of light that you see is the picture of a fixed star. The photograph suggests the desirability of producing star maps by direct photography from the sky.

Here on the screen is a photograph of the great nebula of Orion, or rather a series of photographs of it, made by Mr. Common of Ealing. You will note the gradual development of detail by increase of exposure, and the wonderful amount of detail at last arrived at. Here are photographs from drawings of the same, and you will note the discrepancies between them. And here is a photograph of a star cluster, also by Mr. Common.

No hand of man has tampered with these pictures. They have a value on this account which gives them a distinct and separate claim to confidence above any work in which the hand of fallible man has had a part.

The standpoint of science is so different from that of art. A picture which is a mere copy of nature, in which we do not recognise somewhat of the soul of the artist, is nothing in an artistic point of view; but in a scientific point of view the more absolutely that the individuality of the artist is suppressed, and the more absolutely a rigid representation of nature is obtained, the better.

Here is a volume compiled by one of the most energetic and able of American astronomers—Prof. Holden. It contains faithful reproductions of all the available drawings that have been made by astronomers of this wonderful nebula of Orion from the year 1656 to recent times.

If now we were to suppose one hundred years to elapse, and no further observation of the nebula of Orion to be made in the interval; if in some extraordinary way all previous observations were lost, but that astronomers were offered the choice of recovering this photograph of Mr. Common's, or of losing it and preserving all the previous observations of the nebula recorded in Prof. Holden's book—how would the choice lie? I venture to say that the decision would be —Give us Mr. Common's photograph.

Is it not therefore now our duty to commence a systematic photographic record of the present aspect of the heavens? Will not coming generations expect this of us? Does not photography offer the only means by which, so far as we know, man will be able to trace out and follow some of the more slowly developing phenomena of sidereal astronomy?

Huggins has shown how the stars may be made to trace in the significant cipher of their spectra the secrets of their constitution and the story of their history. Common has shown us how the nebulæ and clusters may be separately photographed, and it is not difficult to see how that process may be applied, not only to special objects, but piece by piece to the whole sky, till we possess a photographic library of each square half-degree of the heavens. But such a work can only be accomplished by consummate instruments, and with a persistent systematic continuity which the unaided amateur is unable to procure and to employ. It is a work that must be taken up and dealt with on a national scale, on lines which Huggins and Common have so well indicated, and which has already been put in a practical form by a proposal of Norman Lockyer's at a recent meeting of the Royal Astronomical Society.

I would that I had the power to urge with due force our duty as a nation in this matter, but my powers are inadequate to the task.

I employ rather the words of Sir John Herschel, because no words of mine can equal those of him who was the prose-poet of our science, whose glowing language was always as just as it was beautiful, and whose judgment in such matters has never been excelled. They were spoken in the early days of exact sidereal astronomy, when the strongholds of space were but beginning to yield the secret of their dimensions to the untiring labour and skill of Bessel, of Struve, and of Henderson. Think what they would have been *now* when they might have told how Huggins' spectroscope had determined the kinship of the stars with our sun, how it had so far solved the mysteries of the constitution of the nebulæ, and pointed out the means of determining the absolute velocity of the celestial motions in the line of sight. Think what Herschel would have said of those photographs by Common that we have seen to-night of that nebula that Herschel himself had so laboriously studied, and whose mysterious convolutions he had in vain endeavoured adequately to portray; and think of the lessons of opportunity and of duty that he would have drawn from such discoveries, as you listen to his words spoken forty-two years ago:—

"Such results are among the fairest flowers of civilisation. They justify the vast expenditure of time and talent which have led up to them; they justify the language which men of science hold, or ought to hold, when they appeal to the Governments of their respective countries for the liberal devotion of the national means in furtherance of the great objects they propose to accomplish. They enable them not only to hold out but to redeem their promises, when they profess themselves productive labourers in a higher and richer field than that of mere material and physical advantages.

"It is then, when they become (if I may venture on such a figure without irreverence) the messengers from heaven to earth of such stupendous announcements as must strike every one who hears them with almost awful admiration, that they may claim to be listened to when they repeat in every variety of urgent instance that these are not the last of such announcements which they shall have to communicate, that there are yet behind, to search out and to declare, not only secrets of nature which shall increase the wealth or power of man, but TRUTHS which shall ennoble the age and country in which they are divulged, and, by dilating the intellect, react on the moral character of mankind. Such truths are things quite as worthy of struggles and sacrifices as many of the objects for which nations contend and exhaust their physical and moral energies and resources. They are gems of real and durable glory in the diadems of princes, and conquests which, while they leave no tears behind them, continue for ever unalienable."

[D. G.]

WEEKLY EVENING MEETING,

Friday, May 30, 1884.

Warren De La Rue, Esq. M.A. D.C.L. F.R.S. Manager and Vice-President, in the Chair.

Monsieur E. Mascart,

PROFESSEUR AU COLLÈGE DE FRANCE.

Sur les Couleurs.

Mesdames, Messieurs,—

C'est une entreprise très hasardée de vous entretenir dans une langue étrangère d'un sujet qui ne vous ménage aucune surprise et dans une salle où vous êtes habitués à entendre les plus grands esprits exposer leurs découvertes. Vous trouverez, sans doute, bien légitime que je réclame toute votre indulgence, dans la crainte surtout que mes honorables amis n'aient à regretter l'invitation qu'ils m'ont fait l'honneur de m'adresser.

Une lumière est définie par deux qualités, l'éclat et la couleur. La comparaison de deux lumières ayant la même couleur pourrait être faite sans le secours de nos organes et par des moyens physiques, mais il est impossible de comparer des couleurs différentes sans faire intervenir l'impression physiologique.

On sait depuis les travaux de Newton que la lumière blanche, ou, pour préciser davantage, la lumière du soleil, est formée d'un grand nombre de couleurs différentes, et que la réunion de toutes ces couleurs dans la même proportion, agissant sur l'œil, soit d'une manière simultanée, soit à des intervalles très rapprochés, reproduit exactement l'impression du blanc.

En partant d'une analogie préconçue avec les notes de la gamme, Newton a partagé le spectre solaire, c'est-à-dire l'image obtenue en décomposant la lumière blanche par un prisme réfringent, en sept couleurs différentes. En réalité, cette division est arbitraire, les couleurs passent de l'un à l'autre par des transitions insensibles, et chacune d'elles peut être caractérisée, soit par sa réfraction dans un prisme, soit plutôt par la longueur des ondulations auxquelles elle correspond.

Quand on réunit en un point une partie des rayons du spectre, on obtient, soit une des couleurs primitives plus ou moins pure, soit une teinte nouvelle. Si on divise le spectre en deux parties arbitraires reunies séparément, on obtient deux couleurs distinctes dont la superposition reproduit encore la lumière blanche. L'expérience peut être réalisée à l'aide d'un spectre ordinaire que l'on divise en deux parties arbitraires; elle s'effectue pour ainsi dire naturellement dans les phénomènes de polarisation rotatoire, où l'on peut obtenir les teintes les plus éclatantes.

Nous ne rappelons ces propriétés générales que pour en déduire deux conclusions. On remarque d'abord que le mélange des lumières simples ou homogènes dans une proportion quelconque produit toujours sur l'œil une impression unique, celle d'une seule couleur; tandis que l'oreille peut distinguer toutes les notes qui constituent une harmonie, l'œil ne saisit jamais qu'une couleur, sans pouvoir distinguer si elle est réellement simple ou formée de lumières différentes.

En second lieu, le mélange des couleurs ne provoque qu'une impression nouvelle, celle du pourpre que l'on peut obtenir, par exemple, en mélangeant du rouge et du violet, les variétés de rose n'étant d'ailleurs que des mélanges de pourpre et de blanc.

Le blanc peut être reproduit par deux couleurs simples seulement, telles que le rouge et le vert; d'une manière plus générale, si l'on isole dans le spectre trois couleurs convenablement choisies, telle que certaines nuances de rouge, de vert et de violet, on peut par leurs mélanges en différentes proportions, imiter les impressions produites par toutes les couleurs. Les couleurs artificielles, formées par des rayons pris dans un spectre par exemple, peuvent être simples ou composées, sans que l'œil en soit informé, sauf peut-être quand elles ont une nuance de pourpre ou de rose, parce que nous savons que ces couleurs n'existent pas à l'état simple.

Il en est de même pour les couleurs de la nature ou de l'industrie. Un objet nous paraît coloré parce qu'il nous renvoie une partie seulement de la lumière qu'il emprunte à l'éclairage général. Le partage se fait soit par transmission, comme dans les verres de couleur, soit par réflexion, comme pour les métaux, soit par diffraction, comme pour les ailes de certains papillons ou les couronnes que l'on distingue quelquefois autour de la lune; la portion de la lumière qui n'arrive pas à l'œil est absorbée ou renvoyée dans une direction différente. Si on laisse à part les effets de fluorescence, on voit qu'un objet n'a pas de couleur par lui-même, il ne fait qu'emprunter à la lumière générale les teintes qui lui conviennent et l'aspect qu'il présente est très différent suivant la mode d'éclairage. Un ruban rouge, par exemple, placé successivement dans les différentes couleurs du spectre paraît noir, sauf dans la région rouge du spectre; il renvoie donc par réflexion une lumière presque homogène. Un ruban rose paraît lumineux d'une manière très inégale dans toutes les parties du spectre; la lumière qu'il réfléchit est donc très complexe.

On peut se demander alors ce que deviendrait la nature, si la lumière qui nous éclaire était absolument homogène. Certains corps l'absorberaient complètement, ils paraitraient obscurs comme du velours noir; d'autres la réfléchiraient plus ou moins activement, ils seraient lumineux avec plus ou moins d'éclat. Comme il n'existerait plus aucun terme de comparaison, l'œil ne conserverait que la sensation du blanc, du noir et des gris intermédiaires.

Il ne semble pas nécessaire d'en faire l'épreuve, mais une expérience n'est jamais inutile et elle révèle presque toujours des idées imprévues.

Pascal disait que rien ne fait mieux comprendre les propriétés de l'air que ce qui se passe là où il n'existe plus ; de même, rien ne fera mieux comprendre les propriétés des couleurs que l'aspect du monde dans un éclairage de lumière homogène. La volatilisation d'un sel de soude dans la flamme d'un bec de Bunsen réalise cette condition d'une manière presque parfaite.

Avec un pareil éclairage les étoffes teintes des plus riches couleurs ne présentent plus que du blanc, du noir et du gris; l'art de la peinture n'existe plus. Voici un superbe tableau d'Ed. W. Cooke, que Sir W. Bowman a eu l'obligeance de me confier, et qui représente un aspect de Venise au soleil couchant ; il n'est plus qu'une gravure : voici maintenant deux paysages dessinés à la lumière de la soude par un artiste qui croyait n'employer que des crayons noirs et gris : il se servait en réalité de couleurs très éclatantes, et vous en verrez l'aspect à la lumière ordinaire; ce bouquet de fleurs n'est plus qu'une collection de taches blanches et grises sur un feuillage noir ; la figure humaine prend un aspect cadavérique, etc. Il faut subir pendant quelque temps l'impression d'un tel spectacle pour saisir complètement ce qu'il a de monotone, de triste et de sépulcral. Ce serait assurément un étrange supplice que d'être obligé à vivre dans un pareil milieu et on éprouverait une joie indescriptible si la baguette d'une fée rendait immédiatement leur éclat à tous les objets qui nous entourent, comme je le fais en enflammant un fil de magnésium, et je suis sûr que vous éprouvez vous-mêmes une sorte de soulagement en voyant la fin de cette expérience lugubre.

Le jugement de la couleur étant lié à l'impression produite sur la rétine, il est à prévoir que l'œil humain ne remplira pas toujours également bien cette fonction.

Déjà les différentes points de la rétine sont inégalement aptes à apprécier les couleurs. Pour distinguer les détails d'un objet, il faut diriger son regard vers cet objet, c'est-à-dire produire une image sur la région centrale de la rétine où l'acuïté de la perception physiologique est beaucoup plus grande. Il en est de même pour les couleurs. Quand on maintient le regard dans une direction déterminée, et qu'on place un corps coloré dans le champ visuel de façon que son image se produise latéralement, on remarque que la notion de la couleur s'affaiblit de plus en plus à mesure qu'on s'écarte de la vision centrale, pour disparaître aux limites du champ.

Mais le fait le plus important consiste en ce que les différentes vues ne distinguent pas les couleurs les unes des autres avec une égale facilité, et même que l'on arrive quelquefois à confondre les couleurs qui nous paraissent les plus discordantes, telles que le vert et le rouge. La découverte de cette forme particulière d'infirmité est due à Dalton, pui en était affecté à un très haut degré, et qui a analysé les erreurs de son jugement avec le plus grand soin. Nous avons l'habitude d'appeler Daltonisme le défaut que vous appelez "colour-blindness," et c'est peut-être abuser du nom d'un savant qui a tant d'autres titres pour passer à la postérité. Ce défaut si longtemps inaperçu est en réalité très fréquent; il y a bien 10 personnes sur 100 qui commettent dans la comparaison des couleurs des erreurs assez marquées pour qu'on puisse les mettre en évidence par un examen attentif. En général, cette imperfection de la vue n'a pas d'inconvénients graves, et on la corrige d'une manière inconsciente, par l'habitude, le souvenir des objets et le jugement d'autrui; mais la gêne devient extrême quand on ne peut distinguer, par exemple, le rouge du vert, une cerise ou une fraise mûre au milieu du feuillage, un feu vert d'un feu rouge dans les signaux des chemins de fer ou de navigation. Les artistes ont quelquefois une prédilection marquée pour certaines couleurs. Lesueur mettait le bleu à profusion dans tous ses tableaux; votre grand peintre Turner semble avoir recherché de plus en plus les tons rouges. Il y aurait lieu peut-être de chercher si le choix de couleurs, pour certains peintres, est absolument intentionnel ou bien s'il est la conséquence d'un état physiologique.

On est presque toujours daltonien de naissance, mais cette affection peut se produire aussi à la suite d'un accident; dans certaines affections nerveuses elle se manifeste quelquefois d'une manière temporaire et sous les formes les plus bizarres.

Plus que les autres sens, la vue peut être ainsi l'occasion d'erreurs et d'illusions nombreuses. Pour ne parler que de celles qui ont trait aux couleurs, je rappellerai les effets de contraste de deux couleurs voisines, ou ceux qui suivent l'impression d'une image, ou encore les couleurs subjectives que l'on voit les yeux fermés, par suite d'une action mécanique sur l'œil, et je me bornerai à vous rendre temoins de quelques expériences relatives au relief apparent des couleurs.

Quand on examine sur un écran l'image d'un spectre, produit par un prisme à vision directe, les couleurs successives semblent bien situées dans un même plan; mais si l'on fait tourner lentement la fente ou le prisme, on a de suite l'illusion d'une lame colorée en relief, dont l'extrémité rouge est en avant. L'effet est plus sensible encore quand la fente a la forme d'un V, le spectre paraît être une véritable gouttière. En remplaçant la fente par le mot DAVY en lettres transparentes, on croirait voir sur le tableau, avec une exagération extrême, les lettres en relief que l'on rencontre sur certaines enseignes de magasins.

En dehors des couleurs que nous voyons habituellement, le spectre solaire renferme encore d'autres radiations, les unes moins réfrangibles que le rouge, qui se manifestent par leurs propriétés calorifiques, les autres plus réfrangibles que le violet, remarquables par leurs effets photographiques, et par l'action qu'elles exercent sur les substances fluorescentes. Le spectre solaire ultra-violet produit par réfraction dans un prisme occupe une étendue à peu près égale à celle du spectre lumineux, et Mr. Stokes a montré que l'arc électrique donne un spectre ultra-violet 5 ou 6 fois plus étendu.

On peut être étonné que la vue humaine soit restreinte à une si faible partie des radiations qu'émet une source de lumière. Nous remarquerons d'abord qu'il en est de même pour les autres sens : le toucher ne peut donner une idée de la temperature d'un corps qu'entre des limites très resserrées ; l'oreille ne percoit pas les sons très graves ni les sons très aigus, et les plus élevés qu'elle puisse entendre produisent une impression pénible. Du côté des rayons infra-rouges, le spectre visible s'arrête presque brusquement, et les efforts de Brewster n'ont guère reculé la limite des rayons que l'œil peut percevoir ; nous devons attendre que des procédés ingénieux, comme ceux que M. le Capitaine Abney a employés avec tant de succès, nous donnent l'histoire complète de cette region.

De l'autre côté, la visibilité persiste d'une manière remarquable. M. Helmholtz avait reconnu déjà qu'avec quelques précautions on peut voir le spectre solaire ultra-violet tout entier, tel qu'il est révélé par la photographie. Ayant eu l'occasion d'étudier la lumière émise par les vapeurs métalliques, j'ai constaté d'abord qu'avec un prisme de spath d'Islande une vue ordinaire peut distinguer un spectre ultra-violet 3 ou 4 fois aussi étendu que le spectre lumineux ; un de mes collaborateurs voyait même beaucoup plus loin et dessinait d'avance toutes les raies brillantes qu'il m'était possible de photographier. Si, au lieu de considérer la réfraction de ces rayons, laquelle varie avec la nature des substances, on les définit par leur longueur d'onde ou la durée de vibration, on peut dire que le spectre lumineux ordinaire comprend l'intervalle d'une octave et qu'il est possible de percevoir une seconde octave plus aigue.

En rappelant ces différentes propriétés de la lumière, je voudrais y ajouter quelques remarques. Sir W. Thomson a exprimé sa surprise que la Nature ait oublié de nous donner un sens particulier pour percevoir les phénomènes magnétiques, au milieu desquels nous vivons. Ici, au contraire, nous sommes en présence de radiations qui n'existent pas dans la lumière du soleil, ou du moins qui n'arrivent pas jusqu'à nous, qui sont énergiquement absorbées par la plupart des milieux transparents et en particulier par les humeurs de l'œil, dont nous nous désintéressons absolument dans la vie courante et qui néanmoins agissent sur la rétine ; la couche nerveuse rétinienne est donc extrêmement sensible à leur action. Ne semble-t-il pas que nous possédions sous ce rapport une sensibilité superflue et qu'il y ait là un défaut d'harmonie entre la structure de l'organe et les besoins auxquels il doit répondre ?

On a soulevé encore, à ce sujet, une question qui présente le plus grand interêt au point de vue philosophique, celle de savoir si l'homme est susceptible d'un perfectionnement organique, et s'il est possible de saisir la trace d'un progrès accompli dans la vision des couleurs, et par suite dans la structure de l'œil. Un des hommes éminents de l'Angleterre n'a pas dédaigné de s'occuper de cette question. Mr. Gladstone a fait un dépouillement complet des expressions employées par Homère pour désigner la couleur des objets; il parait bien en résulter que les épithètes d'Homère sont appliquées d'une manière fort incertaine, que le grand poëte semble confondre le vert avec le jaune, le bleu avec le noir.

Avant de conclure, de cette curieuse observation, que le sentiment de la couleur était alors peu développé, on pourrait remarquer peut-être que l'intervalle qui nous sépare d'Homère est peu de chose dans l'histoire de l'humanité, que les Grecs, quelque temps après, faisaient un grand usage des couleurs dans leurs tableaux et dans les statuettes peintes dont on possède de si nombreux spécimens, que les fresques de Pompeii présentent les couleurs les plus variées, et qu'enfin l'examen attentif des auteurs modernes risquerait de conduire aux mêmes conclusions que celles qu'on tire des écrits d'Homère. Au milieu du 17ᵉ siècle, à l'époque où Lesueur faisait un tel usage du bleu dans la peinture, n'est-il pas singulier qu'un poëte naturaliste par excellence, La Fontaine, n'ait pas employé une seule fois l'expression de "bleu" pour désigner un objet coloré ou la couleur du ciel? Le moindre roman d'aujourd'hui fournirait une récolte de couleurs autrement importante. La littérature avait autrefois pour but de raconter les faits de l'histoire ou les passions de l'homme, afin de produire une impression voulue sur l'esprit du lecteur; la peinture seule se servait du dessin et de la couleur. Depuis quelque temps, au moins en France, il semble que les rôles soient intervertis, la littérature abuse de pittoresque, la peinture devient impressioniste; sans recourir à une modification des nos organes, le changement des idées et le besoin de trouver du nouveau suffiraient pour expliquer le langage coloré des littérateurs contemporains.

D'ailleurs, si l'humanité était capable d'un perfectionnement si rapide, on doit bien admettre que les peuples qui sont restés pour ainsi dire à l'âge de pierre, comme les naturels du Cap Horn, n'ont pas participé au progrès général. L'expédition française qui vient de séjourner pendant une année à la Terre de Feu, a eu l'heureuse idée d'étudier les indigènes sous ce rapport. La langue fuégienne n'a d'expressions que pour désigner deux couleurs, l'un pour le rouge et les teintes analogues, l'autre pour le bleu et le vert; mais cette pauvreté du langage tient seulement à ce que les couleurs ne jouent pas de rôle important dans l'existence des Fuégiens, car on a reconnu qu'avec un peu d'exercice ils arrivaient à distinguer et à classer les couleurs et leur différentes nuances avec autant d'exactitude que l'Européen le plus civilisé; le développement organique de leur appareil visuel ne laisse donc rien à désirer.

La vision des animaux est-elle la même que celle de l'homme, ou bien quelques-uns d'entre eux au moins ont-ils la faculté d'apercevoir des radiations auxquelles nous sommes insensibles? Pour répondre à cette question, nous répéterons d'abord une curieuse expérience de M. Paul Bert. Dans un cuve en verre on a placé de l'eau renfermant une grande quantité de petits crustacés d'eau douce appartenant à la famille des Daphnies. Quand on éclaire un point de la cuve, les daphnies s'y précipitent et elles se déplacent avec le rayon de lumière ; la plupart des animaux sont dans le même cas, et cherchent la lumière à moins qu'elle ne soit trop éclatante. Quand on projette un spectre sur la cuve, on voit encore les daphnies couvrir toute la région éclairée, mais avec des particularités très remarquables. Les plus petites se répandent dans tout le spectre, rares dans le rouge, abondantes dans le jaune et le vert, plus nombreuses dans le bleu et le violet et disséminées encore dans l'ultra-violet. Les plus grosses, au contraire, sont presque exclusivement localisées sur une bande étroite située entre le vert et le bleu. Ces animaux voient donc les même rayons que nous, malgré la distance qui sépare l'homme des crustacés dans l'échelle zoologique, et ils paraissent même partager nos infirmités, puisque quelques-uns se comportent comme s'ils étaient affectés de daltonisme. Sir John Lubbock a fait, dans le laboratoire même de l'Institution Royale, une belle série de recherches sur la vision des fourmis, des abeilles et des guêpes, d'où résulte en particulier ce fait curieux, que les rayons ultra-violets paraissent aux fourmis plus éclatants que le spectre lumineux ordinaire. L'histoire des animaux sous ce rapport serait donc du plus haut interêt.

Nous n'avons considéré jusqu'à présent les couleurs que comme une décoration de la nature, mais leur influence sur le développement des êtres vivants s'exerce dans les conditions les plus variées.

La lumière et les couleurs agissent sans aucun doute sur l'état de notre esprit, et cette impression morale ne peut être que la traduction d'une action physiologique.

Dans certains établissements sanitaires, relatifs aux aberrations mentales, on fait séjourner les malades dans une lumière jaune-dorée qui paraît exercer une heureuse influence sur leur caractère et les amène à des sentiments de douceur. Ce n'est certes pas la lumière jaune de la soude qui peut produire un pareil résultat, mais une sorte de lumière blanche dont on a atténué les rayons extrêmes bleus et rouges de manière à faire dominer les tons roses et jaunes.

La prédilection des animaux pour certaines couleurs n'est pas le résultat d'une préférence artistique. Si les daphnies recherchent la lumière verte et les fourmis la lumière ultra-violette, c'est sans doute qu'elles y trouvent de meilleures conditions d'existence.

Les plantes se prêtent mieux à ce genre d'études. Une plante ordinaire, comme celles que nous avons habituellement sous les yeux, grandit, se développe en tous sens, augmente de poids, produit des feuilles, des fleurs et des fruits, et respire, c'est-à-dire qu'il existe un échange continuel entre les éléments qu'elle contient et les gaz de de l'atmosphère. Ces différents actes de la vie végétative s'effectuent très inégalement sous l'influence des diverses radiations lumineuses ou calorifiques.

La croîssance de la plante, par l'allongement et la multiplication des cellules, se fait surtout sous l'influence des rayons calorifiques, et il existe pour chacune d'elles une température préférée. Si une plante ne reçoit de chaleur que d'un côté, elle s'y développe d'avantage et se courbe dans la direction opposée ; c'est le phénomène du thermotropisme.

A la lumière, une plante s'accroit moins vite que dans l'obscurité, mais au bénéfice de sa nutrition générale et de son développement transversal. Ici les différentes couleurs ont une action spécifique très marquée. Avec un bon éclairage, l'action retardatrice, insensible pour les rayons obscurs, présente un premier maximum vers l'extrémité rouge, un minimum dans le jaune, où la lumière est le plus intense, et un grand maximum dans le violet. Ce sont donc les rayons à courte longueur d'onde qui sont les plus actifs.

Il en résulte l'explication très simple de l'héliotropisme, c'est-à-dire de la tendance marquée des plantes à se courber vers la lumière. Quand une plante est exposée à une lumière latérale, les portions éclairées s'allongent moins vite que celles qui restent dans l'ombre et la plante infléchit la tête vers la lumière. En mesurant cette inflexion dans les différentes régions du spectre, on a reconnu d'ailleurs que l'effet est insensible dans le jaune, plus marqué dans le rouge et qu'il est maximum dans le bleu et le violet.

Nous pouvons entrer plus avant encore dans le mécanisme de la nutrition. En dehors de la perte d'eau par évaporation, les plantes ont deux sortes de respirations : l'une qui est continuelle, le jour et la nuit, et qui dégage de l'acide carbonique, c'est une sorte de combustion corrélative de la vie et tout à fait analogue à la respiration des animaux ; l'autre est intermittente et ne se fait qu'à la lumière, elle a pour résultat d'emprunter à l'acide carbonique de l'air le carbone dont la plante fait du sucre et du bois, et de dégager l'oxygène. La matière colorante des feuilles, la chlorophylle, joue le rôle principal dans cette respiration nutritive. Or, la chlorophylle doit se fabriquer d'abord, puis exercer ses fonctions respiratoires, et ici les différentes couleurs agissent encore très inégalement.

Si l'on examine la formation de chlorophylle dans la plante avec un éclairage moyen, on reconnaît qu'elle se produit dans toute l'étendue du spectre, très faiblement dans l'infra-rouge, avec un maximum dans le jaune intense et une diminution régulière jusque dans le spectre solaire ultra-violet. La courbe de cette action a un allure tout-à-fait analogue à celle qu'a donnée Frauenhofer pour la distribution de l'éclat lumineux dans le spectre, mais elle se prolonge davantage vers les rayons plus réfrangibles. Ici encore il y a une intensité préférée, au-delà de laquelle la chlorophylle se forme moins facilement, pour se détruire ensuite, comme il arrive pour les plantes exposées en plein soleil.

L'expérience montre aussi que la production d'oxygène ne se fait que là où existent les grains de chlorophylle. Si l'on fait traverser une dissolution de chlorophylle par un rayon de lumière blanche qu'on analyse ensuite par un prisme, on remarque une bande d'absorption énergique dans le rouge et deux autres dans le bleu et le violet. Or, ce sont précisément ces rayons absorbés par la substance verte qui agissent pour la réduction de l'acide carbonique, et je ne résiste pas au désir de vous indiquer la méthode ingénieuse employée par M. Engelmann pour mettre ce fait en évidence.

Dans ses expériences sur ses fermentations, auxquelles M. Tyndall a apporté un appui si autorisé, M. Pasteur distingue les petits êtres microscopiques en *aerobies* et *anaerobies;* les uns respirent et se développent en présence de l'oxygène de l'air, l'oxygène tue les autres. Si on examine au microscope des bactéries aerobies nageant dans un liquide, on les voit se concentrer autour des bulles d'air où elles trouvent l'oxygène. Si le liquide est privé de bulles d'air et renferme un filament d'algue verte, les bactéries se promènent indifféremment dans le milieu tant qu'on les éclaire avec une lumière très faible, ou mieux avec une lumière qui a filtré à travers une dissolution de chlorophylle. A la lumière blanche, on voit aussitôt les bactéries se précipiter sur tous les grains de chlorophylle pour y saisir les traces d'oxygène dégagé; elles constituent donc un réactif très délicat.

Pour voir l'effet des différentes couleurs, il suffit maintenant de faire tomber un spectre microscopique sur un filament de conferve ou une coupe transversale de feuille. Les bactéries s'accumulent sur la plante dans le rouge, juste au point où se trouve le maximum d'absorption de la chlorophylle, puis dans le bleu, et la densité de la population, pour ainsi dire, reproduirait à peu près la courbe d'absorption de la matière colorante.

Je ne puis pas insister trop longuement sur ces faits d'histoire naturelle; je dois ajouter cependant qu'il y a sous ce rapport de grandes différences spécifiques, en relation avec la couleur propre des différentes plantes, comme cela doit résulter de l'inégale absorption de leurs matières colorantes. Il me suffira d'en citer un exemple. L'eau de mer a une couleur différente suivant l'épaisseur au travers de laquelle on l'observe, à cause de l'inégale absorption des différentes radiations; suivant la profondeur du sol sur lequel elle repose, une plante marine trouvera donc des conditions plus ou moins favorables, et elle sera plus ou moins bien armée dans sa lutte pour l'existence. Quand on examine une plage que vient de quitter la marée, on trouve des algues bleues sur le bord des plus grandes eaux, plus bas des algues vertes, puis des algues brunes, et enfin des algues rouges dans les régions qui sont rarement découvertes. Du haut d'une falaise, on apercoit ainsi une série de bandes concentriques de couleurs différentes qui dessinent les limites où chacune des espèces, mieux appropriée aux conditions physiques, a éliminé et vaincu les espèces voisines; et ce n'est pas une question de profondeur, parce qu'on retrouve les algues rouges à fleur d'eau dans les endroits abrités, les creux des rochers, les grottes profondes, comme celle de Capri, où la lumière ne peut pénétrer qu'affaiblie. En citant des faits de cette nature, je suis assuré que vous avez tous sur les lèvres le nom d'une des gloires de l'Angleterre et de l'humanité—du naturaliste Darwin.

La lumière est donc une source inépuisable à laquelle les êtres vivants empruntent l'énergie sous toutes les formes et dans les conditions les plus imprévues, et je vous demanderai la permission de terminer cette conférence en citant quelques paroles de Lavoisier, qui, en raison de leur date, paraîtront peut-être une sorte de divination :—

"L'organisation, le sentiment, le mouvement spontané, la vie n'existent qu'à la surface de la terre, et dans les lieux exposés à la lumière. On dirait que la fable du flambeau de Prométhée était l'expression d'une vérité philosophique qui n'avait pas échappé aux anciens. Sans la lumière, la nature était sans vie, elle était morte et inanimée; un Dieu bienfaisant en apportant la lumière, a répandu sur la surface de la terre l'organisation, le sentiment et la pensée."

[E. M.]

GENERAL MONTHLY MEETING,

Monday, June 2, 1884.

Sir Frederick Pollock, Bart. M.A. Manager and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:

Sir Frederick Bramwell, F.R.S.
Warren De La Rue, Esq. M.A. D.C.L. F.R.S.
The Hon. Sir William R. Grove, M.A. D.C.L. LL.D. F.R.S.
Sir John Lubbock, Bart. M.P. D.C.L. LL.D. F.R.S.
Sir Frederick Pollock, Bart. M.A.
The Earl of Rosse, D.C.L. LL.D. F.R.S.
George Busk, Esq. F.R.S. Treasurer.
Sir William Bowman, Bart. LL.D. F.R.S. Honorary Secretary.

James Mansergh, Esq. M.I.C.E.

was elected a Member of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Palæontologia Indica: Series XIV. Vol. I. Part 3, Fasc. 3. 4to. 1884.
Records, Vol. XVII. Part 2. 8vo. 1884.

The French Government—Documents Inédits sur l'Histoire de France:
Mélanges Historiques, Tome IV. 4to. 1882.
Lettres de Jean Chapelain. Par P. Tamizey de Labroque. Tome II. 4to. 1883.
Inscriptions de la France. Par F. de Guilhermy et R. de Lasteyrie. Tome V. 4to. 1883.
Monographie de Notre Dame de Chartres. Explication des Planches par P. Durand. 4to. 1881.
Lettres du Cardinal Mazarin. Par A. Chéruel. Tome III. 4to. 1883.
American Philosophical Society—Proceedings, No. 114. 8vo. 1883–4.
Asiatic Society of Bengal—Proceedings, 1883, No. 10; 1884, No. 1. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. XLIV. No. 6. 8vo. 1884.
Ateneo Veneto—Rivista Mensile, Serie VII. Nos. 3–6; Serie VIII. Nos. 1, 2. 8vo. 1883–4.
Aubertin, J. J. Esq. M.R.I. (the Translator)—The Lusiads of Camoens. Translated into English Verse. 2 vols. 8vo. 1884.
Bagot, Alan, Esq. M.I.C.E. (the Author)—Application of Electricity to the Working of Coal Mines. (Proc. Inst. Mech. Engineers, 1883). 8vo.
Bankers, Institute of—Journal, Vol. V. Part 5. 8vo. 1884.
British Architects, Royal Institute of—Proceedings, 1883-4, Nos. 12–14. 4to.
British Association for the Advancement of Science—Report of Meeting at Southport, 1883. 8vo. 1884.
Chemical Society—Journal for May 1884. 8vo.
East India Association—Journal, Vol. XVI. No. 3. 8vo. 1884.
Editors—American Journal of Science for May 1884. 8vo.
Analyst for May 1884. 8vo.
Athenæum for May 1884. 4to.
Chemical News for May 1884. 4to.
Engineer for May 1884. fol.
Horological Journal for May 1884. 8vo.
Iron for May 1884. 4to.
Nature for May 1884. 4to.
Revue Scientifique and Revue Politique et Littéraire for May 1884. 4to.
Science Monthly, Illustrated, for May 1884.
Telegraphic Journal for May 1884. 8vo.
Franklin Institute—Journal, No. 701. 8vo. 1884.
Geddes, Patrick, Esq. (the Author)—A Re-Statement of the Cell Theory. (Proc. Roy. Soc. of Edin. XII.) 8vo. 1884.
Geographical Society, Royal—Proceedings, New Series, Vol. VI. No. 5. 8vo. 1884.
Geological Society—Quarterly Journal, No. 158. 8vo. 1884.
Gordon, Surgeon-General C. A. M.D. C.B. M.R.I. (the Compiler)—Medical Reports of the Chinese Customs Service, 1871-1882. 4to. 1884.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Tome XVIII. Liv. 2, 3, 4, 5; Tome XIX. Liv. 1. 8vo. 1883–4.
Natuurkundige Verhandelingen. 3de Verz. Deel IV. 3de Stuk. 4to. 1883.
Programme for 1882, 1883. 4to.
Johns Hopkins University—American Chemical Journal, Vol. VI. No. 2. 8vo. 1884.
University Circulars, No. 30. 4to. 1884.
Linnean Society—Journal, Nos. 130, 131. 8vo. 1884.
Manchester Geological Society—Transactions, Vol. XVII. Part 15. 8vo. 1883–4.
Manchester Literary and Philosophical Society—Memoirs, 3rd Series, Vols. VII. and IX. 8vo. 1882-3.
Proceedings, Vols. XX. XXI. XXII. 8vo. 1880–3.
Meteorological Office—Barometer Manual for Seamen. 8vo. 1884.
Report of Meteorological Council, R.S. to 31st March, 1883. 8vo. 1884.
Monthly Weather Report for January 1884. 4to.
National Association for Social Science—Proceedings, Vol. XVII. No. 2. 8vo. 1884.

National Lifeboat Institution, Royal—Annual Report for 1884. 8vo.
Pharmaceutical Society of Great Britain—Journal, May 1884. 8vo.
Photographic Society—Journal, New Series, Vol. VIII. No. 7. 8vo. 1884.
Physical Society—Proceedings, Vol. V. Part 5. 8vo. 1884.
Preussische Akademie der Wissenschaften—Sitzungsberichte, I.-XVII. 4to. 1884.
Royal Society of Literature—Transactions, Vol. XIII. Part 1. 8vo. 1883.
Royal Society of London—Proceedings, No. 230. 8vo. 1884.
Royal Society of New South Wales—Journal of Proceedings, Vol. XVI. 8vo. 1883.
Society of Arts—Journal, May 1884. 8vo.
Society for Psychical Research—Proceedings, Vol. I. Part 5. 8vo. 1884.
Teyler Museum—Archives, Série II. 4e Partie. 4to. 1883.
Toronto Observatory—Report of the Canadian Observations of the Transit of Venus, 6th Dec. 1884. 8vo.
United Service Institution, Royal—Journal, No. 123. 8vo. 1884.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1884: Heft 4. 4to.
Wild, Dr. H. (the Director)—Annalen des Physikalischen Central-Observatoriums, 1882, Theil II. 4to. 1883.

WEEKLY EVENING MEETING,

Friday, June 6, 1884.

Warren De La Rue, Esq. M.A. D.C.L. F.R.S. Manager and Vice-President, in the Chair.

Willoughby Smith, Esq. *M.R.I.*

Volta-Electric and Magneto-Electric Induction.

The subject which I shall bring before your notice this evening is "Volta-Electric and Magneto-Electric Induction"; and I propose to describe some of my experiments in connection with this phenomenon in electricity and magnetism, which was discovered, named, and for forty years fostered within this very building by our universally esteemed and beloved Michael Faraday.

It is now thirty years ago that Faraday gave me my first lesson in Volta-Electric Induction, at the same time impressing upon me the fact that physical science must ever be progressive and corrective, and that he was always pleased to learn that his experiments had been repeated by others, with a view to their verification or correction. In the remembrance of this, I have felt encouraged in coming before you this evening.

Doubtless all present are familiar with the fact, that about 1819 Professor Oersted made a discovery which has done much to advance our knowledge with regard to electricity and magnetism. This discovery arose from a very simple experiment, but it is none the less valuable on that account. I will now repeat this experiment, as the result obtained will lead up to, and enable you to better understand what I may show later on. Here is a length of copper wire, from the top surface and centre of which projects a metal pin, having, balanced on its point, a magnet. The position of the steel magnet is, as you perceive, parallel with the wire; and that position being north and south, it will thus remain until, by pressing down this spring, the length of copper wire is placed in metallic circuit with this battery, and you observe that the magnet immediately has a tendency to place itself at right angles to the copper wire, remaining thus displaced until the current ceases to flow, when the needle again obeys the influence of terrestrial magnetism, and returns to its former position. This was Oersted's experiment which led to the discovery that all bodies possess the qualities of a magnet whilst a current is passing through them. From this fact Faraday conceived the idea that, if a similar length of copper wire were placed parallel with the former one, and close to it, each time the circuit was made or unmade there would be an induced current flowing in this wire, provided it was part of a closed metallic or other conducting circuit. The first experiment was not successful, for the simple reason that the galvanometer was not sufficiently sensitive to be visibly affected by the very small current induced in so short a length of wire as that with which he made the experiment; nor was he prepared to find that induced currents were of such momentary duration. But failures with Faraday were merely stepping-stones to success; he repeated the experiments with spirals of insulated wire, instead of straight wires parallel to each other, thus getting comparatively long lengths in close proximity. By these means he gained the object of his search, the result of which he called "Volta-Electric Induction." Through the kindness of Dr. Tyndall, I have here the identical spirals which were made and used by Faraday on that memorable occasion. One of these Faraday connected in circuit with a galvanometer, and placed it on top of the other spiral, through which intermittent currents from a battery were sent at fixed intervals. On "making" the battery circuit, he noticed that the needle of the galvanometer was deflected in one direction, and on breaking the circuit, the needle was again deflected, but in the opposite direction. Faraday saw, in his mind's eye, each particle of the circuit through which the current was passing, acting as a centre of force, emitting its lines far from it, yet each of these lines returning to its own source; he in consequence made a series of experiments, by placing various substances in the path of the lines of force, to ascertain whether they would in any way be affected or intercepted by the substances so placed. For instance, he found that they were sensibly affected by iron, but with copper no satisfactory effects were perceived, although he felt sure the copper did in some way influence them, but so imperceptibly that he was unable to detect it. It was this conviction, and the doubt by Faraday of the result of his experiment with regard to copper, which led me to experiment in this direction. My apparatus and its arrangements being somewhat different from Faraday's, I will more fully describe them. Here are two flat spirals of fine silk-covered copper wire, about twelve inches in diameter, suspended spider-web fashion in separate frames, the two ends of each spiral being attached to terminals at the base of its own frame. These two spirals, which are marked respectively A and B, will now be placed a definite distance apart, and comparatively slow reversals from a battery of ten cells sent through spiral A. You will see the amount of the current induced in B by observing the deflection on the scale of the mirror reflecting galvanometer, which is in circuit with that spiral. These inductive effects vary inversely as the square of the distance between the two spirals when parallel to each other; the induced current in B being also proportional to the number of reversals of the battery current passing through spiral A, and also to the strength of the inducing current. Spiral A is so connected that reversed currents, at any desired speed per minute, can be passed through it from a battery; B is so connected to the galvanometer and a reverser as to show the deflections caused by the induced currents, which are momentary in duration, and, in the galvanometer circuit, all on the same side of zero; for, as the battery current, on making contact, produces an induced current in the reverse direction to itself, but in the same direction when broken, of course the one would neutralise the other, and the galvanometer remain unaffected. To obviate this, the galvanometer connections are reversed with each reversal of the battery current, and thus a steady deflection is produced.

Perhaps, for the information of those not acquainted with the construction of a galvanometer, I ought to explain it more fully. The one I am about to use consists of a coil of very fine silk-covered wire, in the centre of which is suspended a very small magnet; the ends of the coil of wire and the ends of spiral B are connected respectively together, thus forming a metallic circuit, one part of which is wound into a coil and the other into this spiral. Call to mind Oersted's experiment, and it will be readily understood that when a current of electricity is flowing in this metallic circuit, the magnet will be influenced in magnitude according to the amount of current thus flowing. The movements of the magnet, however, would be too small to be seen unless watched very closely; therefore fixed to it is a very small concave mirror, on to which a beam of light from the lamp is thrown, and the mirror reflecting this on to the scale, will, I hope, enable all to see that somewhat broad beam move in accordance with the movements of the magnet. Reversed currents at the speed of 100 per minute will now be passed through spiral A, and you will observe that the induced currents in B give about 28 divisions on the scale of the galvanometer: we will note this on the black-board. Now, we place this plate of iron midway between the two spirals, and you observe the deflection on the scale is reduced to about one-half, or in round numbers to 15, showing clearly that the presence of the iron plate has in some way influenced the previous effects. We now remove the iron, when you see the deflection returns to its original amount of 28 divisions; and if I now interpose a similar sheet of copper, the interposition does not alter the deflection. The results of this experiment are therefore as follows:—

Speed = 100 reversals per minute.
Induced current = 28° deflection.
Iron interposed = 15° „
Copper „ = 28° „

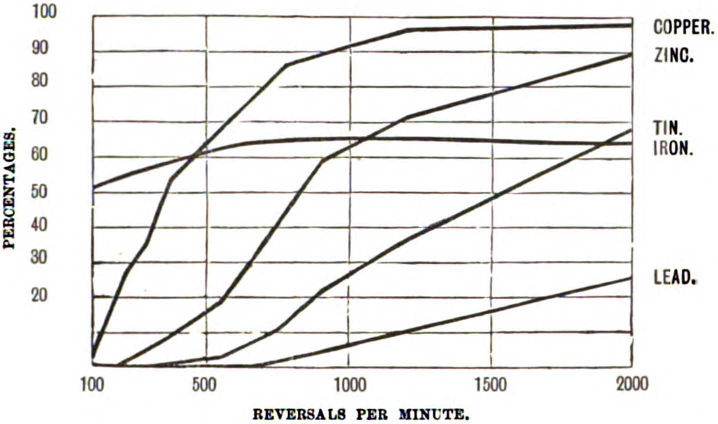
I may here state that, up to this point, the results of my experiments confirm those of Faraday, viz. that all dielectrics and diamagnetic metals appear in no way to interrupt or interfere with the lines of force.

Now, let us repeat this experiment with the speed of the reversals increased ten times, or to 1000 per minute; the spirals are in the same position as before, and the deflection is now about 86. I have already said that the induced current is in direct proportion to the speed of the reversals, the battery and spirals remaining the same. It might therefore cause confusion were I not to explain that the deflection would be ten times as great as it was with the lower speed of reversals, but that the scale of the galvanometer not being sufficiently long to record this high deflection, we have what is termed "shunted" a part of the current; that is to say, between the two ends of the galvanometer coil we have inserted a length of copper wire, so that the current, on arriving at one terminal of the coil, divides, part going through the coil of the galvanometer, and the rest through the shunt; the two currents reunite at the other terminal of the galvanometer. By varying the resistance of the shunt, therefore, the desired amount of the current can be sent through the galvanometer. In this case sufficient current passes to keep the beam of light just on the scale at about 86 divisions. We now interpose the sheet of iron as before, and you see the deflection falls as before to about one-half. We withdraw the iron and the deflection returns to its former amount of 86. We now interpose the copper, when the deflection, instead of remaining stationary, as in the former experiment, actually falls to 17. We now obtain the following results, viz.:—

Speed = 1000 reversals per minute.
Induced current = 86° deflection.
Iron interposed = 40° „
Copper „ = 17° „

Now, the question arose, why does copper, at the low speed of the reversals, apparently have no effect, while at the higher speed it plays so important a part in intercepting the lines of force? The only solution of the phenomenon which suggested itself to me was that the lines of force have first to polarise the molecules of substances placed in their path before they can pass through them, and in this process time is a very important element to be considered; for instance, at the slow speed there is sufficient time between the reversals for the copper to polarise before the next reversal takes place; whereas at the high speed the copper plate is unable to fully polarise before the next reversal arrives, and then the two induced effects partly blend, and being opposite in direction tend to cancel each other. Now, if this really be the case, the higher the speed the less should be the proportional deflection when experimenting with copper. The time at our disposal this evening will not allow of accurate measurements, or of other substances being experimented upon, but careful and reliable measurements have been made, the results of which are shown on the sheet before you marked 1. It will be seen by reference to these results that the percentage of inductive energy intercepted does not increase for different speeds of the reverser in the same rate with different metals, the increase with iron being very slight, whilst with copper the induced current set up is so long in duration that when the speed of the reverser is at all rapid, the current not having time to exhaust itself before the galvanometer is reversed, tends to produce a lower deflection. If the speed of the reverser is further increased, the induced current is received on the opposite terminal of the galvanometer, and thus a negative result is obtained.

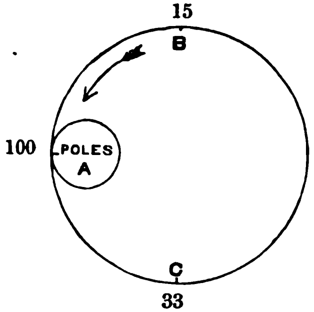
PLATE 1.



My next object was to verify, if possible, by a different system of experiment, the correctness of this theory, and I could think of no better arrangements than those used by Faraday in some of his experiments on Magneto-Electric Induction. I was not, however, encouraged to proceed in that direction; for if my theory were correct, the results published by Faraday could not be so; and knowing what a careful experimentalist he was, I could not doubt that he was right. After long and careful thought on the subject, I ventured, however, to repeat some of his experiments, and I will again repeat them before you presently.

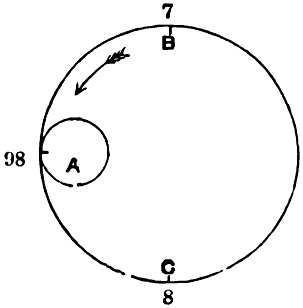
About sixty years ago Arago made the discovery in Electrical Science, that if a plate of copper be revolved close to a magnetic needle, or magnet suspended in such a way that the latter may rotate in a plane parallel to the former, the magnet tends to follow the motion of the plate; or if the magnet be revolved the plate tends to follow its motion. This simple apparatus will better illustrate the experiment. Here is a copper plate one-tenth of an inch thick, and seven and a half inches in diameter, fixed to a vertical spindle and enclosed in a wooden case having a glass cover; beneath the copper plate is a small grooved pulley around which passes an endless band; the band also passing round this horizontal wheel to which a handle is fixed, so that it may be conveniently revolved. A small brass disc is here provided, in the centre of which is fixed a pointed steel pin, and on this pin is balanced a steel magnet. I will place this on the glass cover over the centre of the copper plate. Now, if the copper plate be made to revolve, you will see that the magnet will revolve also in the direction of the copper disc. There it goes! No doubt you observed how sluggish its movements were at first, and that it was some time before it followed the movements of the disc; this was owing to the attraction of the earth's magnetism on the magnet, which held it in bondage until the speed of the disc was sufficient to overcome its attraction, then, once released, how merrily it appeared to obey the influence of a superior power. The disc now being at rest, the needle has returned into bondage; but if I judiciously use the influence of this magnet to partially neutralise the influence of the earth's attraction, you will observe how much more quickly it obeys the influence of the mysterious power of the revolving disc. There it goes! Apparently more readily than before. Were I lecturing on moral philosophy, I certainly should make use of the similes which might be drawn with advantage from experiments with this simple instrument; but my subject being of a different nature, I will resume without further digression. If the order were reversed and the magnet revolved, the copper disc would act in the same way as the magnet has just done. This is the phenomenon discovered by Arago, who also asserted that the effect takes place, not only with all metals, but with all substances. On this latter point there has always been a difference of opinion between experimentalists who have endeavoured to verify Arago's statement. As far as my experiments have gone, I have only obtained reliable results from good conductors of electricity; but I believe that, theoretically, Arago is right; for as all substances are, in a certain degree, conductors of electricity, it, I think, necessarily follows that we only want sufficiently sensitive instruments to develop the phenomenon, as Arago asserts, in every substance. It has been stated that all substances when subjected to a sufficiently strong magnetic force are found to give indications of polarity, and also, when magnetic force acts on any medium, whether magnetic, diamagnetic, or neutral, it produces within it a phenomenon called magneto-induction. If this be the case it materially strengthens the correctness of Arago's assertion. Arago's discovery that copper, a non-magnetic metal, was influenced by a rotating magnet, or that a magnet was, in the same way, affected by a rotating disc of copper, was looked upon at the time as a remarkable and new phenomenon in induced magnetism by philosophers both in England and other countries; but it was Faraday who, by the assistance of his knowledge of the evolution of electricity from magnetism, gave the true solution, by proving it to be the effect of electrical currents induced in the disc on account of its motion in a magnetic field. He not only proved by simple experiments the correct interpretation of the phenomenon, but he saw the way to making the discovery of Arago a new source of electricity, not despairing, by the aid of his knowledge of terrestrial-magneto-induction, of being able to construct a new magneto-electric machine. For this purpose he arranged an apparatus similar to the one I have here, which is simply a permanent magnet, so fixed that discs of metal or other substances can be rotated between its poles. Two wires leading one from each terminal of a galvanometer were applied to any desired part of the revolving disc, and the deflections on the galvanometer noted. The first experiments were made with a very large compound permanent magnet, and with what would now be termed a quantity astatic galvanometer. His discs of metal were twelve inches in diameter, and about one-fifth of an inch in thickness, fixed upon a brass axis. He experienced difficulty in making contact between the terminals of the galvanometer and the edge and other parts of the revolving disc, as also in maintaining uniform velocity of rotation. With a much smaller magnet and a more sensitive galvanometer, the results were more striking. Thus, with his accustomed simplicity, he demonstrated the production of a permanent current of electricity from an ordinary magnet, at the same time asserting that with powerful magnets and rapid rotation of a copper disc, very strong currents would be produced. What a wonderful lesson this apparently simple machine teaches, for it is able to exert the power, which has its origin within itself, on external matter, without in any way exhausting or diminishing that power!

The apparatus that I employed for my experiments is the same that I shall use this evening. On this stand is fixed an electro-magnet, the poles of which are so placed that the rim of the disc under experiment can be freely revolved between them. The cores of the electro-magnet are 1·75 inch in diameter, and are wound with twelve layers of silk-covered copper wire of high conductivity, ·028 of an inch in diameter, each layer having sixty-one turns; each core has 732 turns, making a total of 1464 turns on the two poles. The total resistance of the wire is 13·75 Ohms, and through this flows the current from twelve Leclanché cells. The same galvanometer is used as in the other experiments, being brought in circuit with the disc by means of the axis on which the disc revolves, and a metal brush which forms a rubbing contact on the rim of the disc; this, working on a fixed centre, can be readily shifted to any part of the circumference of the revolving disc. The disc is revolved by this cone-shaped pulley, worked lathe fashion, and connected by an endless band to a small grooved pulley fixed on the same axis as the metal disc. We will now make a few experiments with the copper disc, which is now revolving at the speed of 1000 revolutions per minute, and the connections are so made that the current will be taken from the rim of the disc just as it passes between the poles of the magnet. On pressing this spring the circuit is completed, and you observe the effect of the current on the scale of the galvanometer; it is about 100 divisions. We will now remove the contact to about one-fourth of the diameter of the disc or top position, so that the current will be taken at that distance from the poles of the magnet as the disc is approaching the poles; now, on completing the circuit, you observe the deflection is reduced to about fifteen divisions. The contact will now be removed to the opposite side, or bottom position of the disc, so that the current will be taken at the same distance from the poles, but after that part of the disc has just passed between them; on completing the circuit you see the deflection has increased to 33 divisions. Let us note these results as follows:—



Copper.

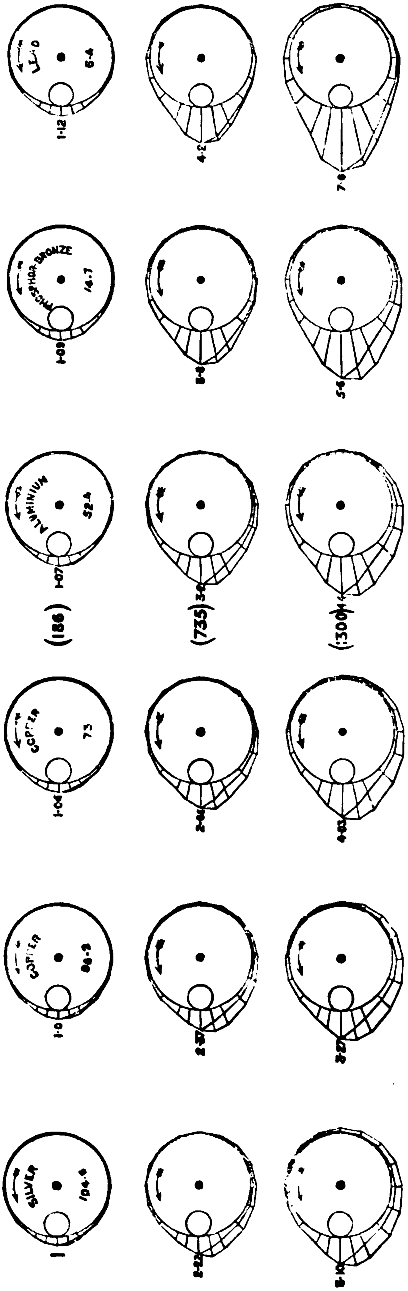
We will now replace the copper disc with one of iron, and repeat precisely the same experiment. We find that the current from position A is so great that it is necessary to insert a resistance of 500 Ohms in the circuit to bring the beam of light on to the galvanometer scale: as before shown, by means of inserted resistance we can obtain any desired deflection. It is now about 98, and that we will take as the right measure at A. We take the top position, or B, and you observe that the deflection is only 7 divisions; the connection is now placed at position C, or bottom part, and we get but very little more current as the deflection is only increased by one division. The results with iron are therefore as follows:—



Iron,
500 Ohms in circuit.

If we compare the results of these experiments with those obtained with the same metals in volta-electric induction, we see that iron remains fairly uniform in its results in both cases, but that copper behaves differently. You must not, please, take the figures given as being absolutely correct, the experiments having been somewhat hurriedly made, and merely given with the view of enabling you to understand the results of carefully-made experiments with various metals. These results are graphically shown on the large sheet suspended before you, marked 2; and, as the measurements are to scale, they can be compared directly with each other. The large circles represent the revolving discs, the small ones show the position of the poles of the electro-magnet, and the arrows the direction of rotation. The coloured portions show the electro-motive force at every point round the revolving disc; thus in every case the strongest point is, as might be expected, directly in front of the poles of the exciting magnet, the strength gradually fading from thence on either side. The blue portions show the excess of inductive effect there produced, relatively to a point equidistant from the magnetic poles, but on the opposite side. If the current set up in the metal simply depended upon the intensity of that part of the magnetic field through which the metal was passing, then equidistant points on either side of the magnetic poles should produce equal deflections, because the magnetic intensity is equal; on inspecting the diagrams,

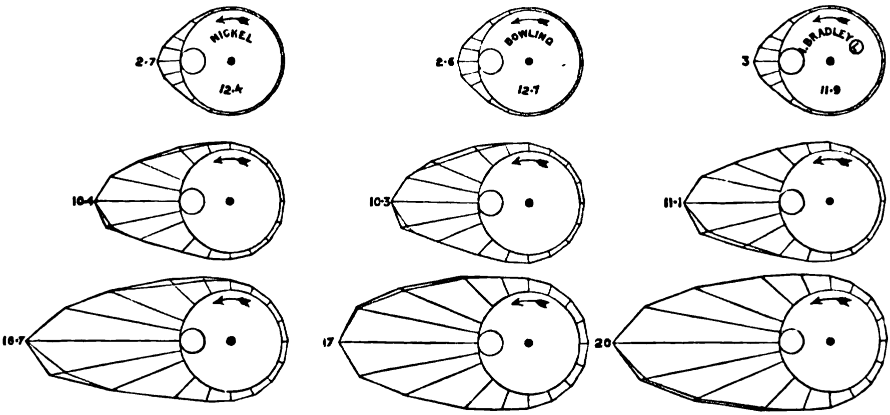
Plate 2.



however, it appears as if a very sluggish inductive effect were produced in the diamagnetic metals of high conductivity, the sluggishness increasing with the conductivity of the metal, as though the atoms took a comparatively long time to accommodate themselves to the changes in the magnetic field through which they were passing. This, I think, confirms the theory I have ventured to advance with respect to copper while under the influence of induction.

If, as in the case of volta-electric induction, the lines of force are generated too quickly; or, as in the case of magneto-electric induction, the diamagnetic body passes too quickly through the magnetic field; the atoms of the substance, in each case, have not time to polarise with sufficient rapidity to radiate in the same time or place as when influenced more slowly by this force. This would account for the apparent interruption of the lines of force by the copper plate in the volta-electric experiments, and also for the "drag" or retardation on the disc of the same metal, as shown by the blue colour in the diagram whilst part of it is passing quickly through a magnetic field. That being the case, it is easy to perceive that a speed of the copper disc might be attained at which the current would be *nil*, through the atoms being unable to polarise in the allotted time. Not so with magnetic metals, as iron or nickel, in each of which the atoms appear to be very susceptible to magnetic influences, and to polarise very quickly, as shown by the way in which part of the disc is affected just before entering the poles, as indicated by the blue colour in the diagram marked 3.

PLATE 3.

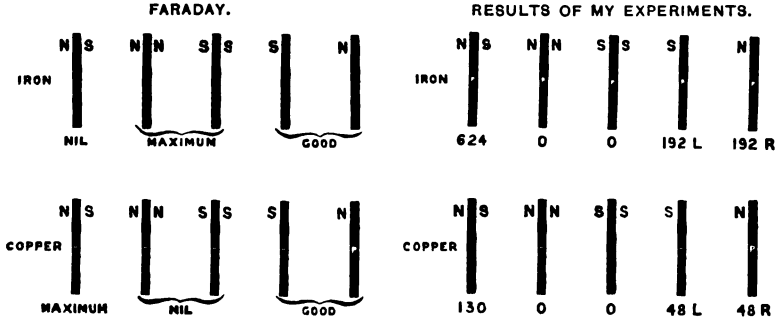


The atoms must as quickly depolarise, as you perceive there is but very little of what I have termed "drag" in those metals. The consequence of this is, that with iron the amount of current is nearly in direct proportion to the speed of the disc; whereas with copper the increase is very small, the current produced by the two metals being as 1 to 7, at a speed of about 1300 revolutions per minute. The higher the specific conductivity of the diamagnetic metal, the greater the "drag," and consequently the less the current at the poles, this being more manifest at the high speeds, which does not, however, agree with the results obtained by Faraday, for he found that the currents were proportionate in strength to the conducting power of the bodies experimented with, or, in other words, that the higher the conductivity the greater the current, whereas I find the reverse to be the case, as shown on the diagram before you.

Faraday also obtained much better results from copper than from iron, and thus he recommended copper for his new magneto-electric machine. Here, again, my results do not agree with his, for I find that iron gives much better results than copper, as shown on the diagram; and also that iron has the advantage that the current increases almost in direct proportion to the speed, whereas copper does not; in fact, as already stated, a speed might be obtained at which copper would give no current.

Faraday, in summing up the results of his experiments on Arago's phenomenon, says: "Nothing can be more clear, therefore, than that with iron and bodies admitting of ordinary magnetic induction, opposite poles on opposite sides of the edge of the plate neutralise each other's effects, while similar poles exalt the action. But with copper, and substances not sensible to ordinary magnetic impressions, *similar* poles on opposite sides of the plate neutralise each other, and opposite poles exalt the action." Perhaps you will more readily grasp the subject by reference to the diagram marked 4, in which P represents the plates of metal, and the other letters the respective poles of the magnet and their position.

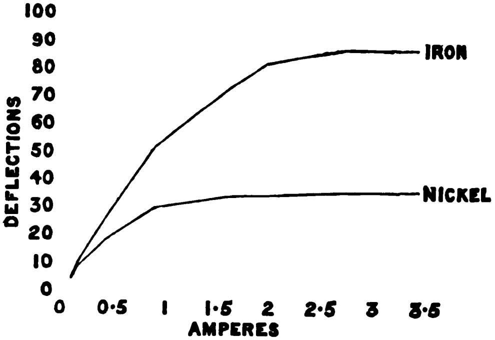
Plate 4.



Here again it will be seen that the results obtained by me are opposed to those given by Faraday. I have given the actual figures obtained in my experiments, and it will be seen that the only difference between the metals copper and iron is that iron gives the higher current of the two, as it has done in all my experiments.

I have no doubt that we should be able to get approximately near for all practical purposes to the relative conductivity of metals by revolving discs of the metal under test in a magnetic field, and measuring the amount of current between the poles or the amount of "drag." The results given on diagram 5 show how suitable the method also is for obtaining the saturation point of the cores of electro-magnets, whether of different qualities of iron or different metals.

PLATE 5.



Very recently an Electrical Congress, held in Paris, agreed to an universal system of electrical units. I believe the value of some of the units to be adopted was determined by passing masses of metal at varying velocity through a magnetic field of uniform intensity, and noting the amount of current so produced. I understand that the results obtained by different experimentalists did not agree, and from what I have shown you this evening, you will readily understand the reason of the discrepancy. To obtain accurate results the influence of what I have called "drag" must certainly be taken into the calculations as an important factor. There are other matters to which I attach importance connected with the experiments I have endeavoured to make clear to you, especially with those on magneto-electric induction, which I publish for the first time this evening; but of the many good rules of this Institution, there is one which does not allow me to tax your patience for more than one hour, and, as I have already exhausted that time, I must not detain you longer, except to thank you, which I do most sincerely, for the kind attention you have given to my humble endeavours to advance our knowledge of volta-electric and magneto-electric induction.

[W. S.]

WEEKLY EVENING MEETING,

Friday, February 8, 1884.

Sir William Bowman, Bart. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

George J. Romanes, Esq. M.A. LL.D. F.R.S.

The Darwinian Theory of Instinct.

"Gavest thou the goodly wings unto the peacocks? or wings and feathers unto the ostrich? which leaveth her eggs in the earth, and warmeth them in dust, and forgetteth that the foot may crush them, or that the wild beast may break them. . . . Because God hath deprived her of wisdom, neither hath He imparted to her understanding."

This is the oldest theory of instinct. The writer of that sublime monument of literary power in which it occurs observed a failure of instinct on the part of the ostrich, and forthwith attributed the fact to neglect on the part of the Deity; the implication plainly being that in all cases where instinct is perfect, or completely suited to the needs of the animal presenting it, the perfection is to be attributed to a God-given faculty of wisdom. This, I say, is the oldest theory of instinct, and I may add that until within the past twenty-five years it has been the only theory of instinct. I think, therefore, I ought to begin by explaining that this venerable and time-honoured theory is a purely theological explanation of the ultimate source of instinct, and therefore cannot be affected by any scientific theory as to the proximate causes. It is with such a theory alone that we shall here be concerned.

"When giants build, men must bring the stones." For the past eight or ten years I have been engaged in elaborating Mr. Darwin's theories in the domain of psychology, and I cannot allude to my own work in this connection without expressing the deep obligations under which I lie to his ever ready and ever generous assistance—assistance rendered not only in the way of conversation and correspondence, but also by his kindness in making over to me all his unpublished manuscripts, together with the notes and clippings which he had been making for the past forty years in psychological matters. I have now gone carefully through all this material, and have published most of it in my work on 'Mental Evolution in Animals.' I allude to this work on the present occasion in order to observe that, as it has so recently come out, I shall feel myself entitled to assume that few have read it; and therefore I shall not cramp my remarks by seeking to avoid any of the facts or arguments therein contained.

As there are not many words within the compass of our language which have had their meanings less definitely fixed than the word "instinct," it is necessary that I should begin by clearly defining the sense in which I shall use it.

In general literature and conversation we usually find that instinct is antithetically opposed to reason, and this in such wise that while the mental operations of the lower animals are termed instinctive, those of man are termed rational. This rough and ready attempt at psychological classification has descended to us from remote antiquity, and like kindred attempts at zoological classification, is not a bad one so far as it goes. To divide the animal kingdom into beasts, fowls, fish, and creeping things, is a truly scientific classification as far as it goes, only it does not go far enough for the requirements of more careful observation; that is to say, it only recognises the more obvious and sometimes only superficial differences, while it neglects the more hidden and usually more important resemblances. And to classify all the mental phenomena of animal life under the term "instinct," while reserving the term "reason" to designate a mental peculiarity distinctive of man, is to follow a similarly archaic method. It is quite true that instinct preponderates in animals, while reason preponderates in man. This obvious fact is what the world has always seen, just as it saw that flying appeared to be distinctive of birds, and creeping of reptiles. Nevertheless, a bat was all the while a mammal, and a pterodactyl was not a bird; and it admits of proof as definite that what we call instinct in animals occurs in man, and that what we call reason in man occurs in animals. This, I mean, is the case if we wait to attach any definition to the words which we employ. It is quite evident that there is some difference between the mind of a man and the mind of a brute, and if without waiting to ascertain what this difference is, we say that it consists in the presence or absence of the faculty of reason, we are making the same kind of mistake as when we say that the difference between a bird and a mammal consists in the presence or absence of the faculty of flying. Of course, if we choose, we may employ the word "reason" to signify all the differences taken together, whatever they may be; and so, if we like, we may use the word "flying." But in either case we should be talking nonsense, because we should be divesting the words of their meaning, or proper sense. The meaning of the word "reason" is the faculty of ratiocination—the faculty of drawing inferences from a perceived equivalency of relations, no matter whether the relations involve the simplest mental perceptions, or the most abstruse mathematical calculations. And in this, the only real and proper sense of the word, reason is not the special prerogative of man, but occurs through the zoological scale at least as far down as the articulata.

What then is to be our definition of instinct?

First of all, instinct involves *mental* operation, and therefore implies *consciousness*. This is the point which distinguishes instinct from reflex action. Unless we assume that a new-born infant, for example, is conscious of sucking, it is as great a misnomer to term its adaptive movements in the performance of this act instinctive, as it would be similarly to term the adaptive movements of its stomach subsequently performing the act of digestion.

Next, instinct implies hereditary knowledge of the objects and relations with respect to which it is exercised; it may therefore operate in full perfection prior to any experience on the part of the individual. When the pupa of a bee, for instance, changes into an imago, it passes suddenly from one set of experiences to another—the difference between its previous life as a larva and its new life as an imago being as great as the difference between the lives of two animals belonging to two different sub-kingdoms; yet as soon as its wings are dry it exhibits all the complex instincts of the mature insect in full perfection. And the same is true of the instincts of vertebrated animals, as we know from the researches of the late Mr. Douglas Spalding and others.

Again, instinct does not imply any necessary knowledge of the relation between means employed and ends attained. Such knowledge may be present in any degree of distinctness, or it may not be present at all; but in any case it is immaterial to the exercise of the instinct. Take, for example, the instinct of the Bembex. This insect brings from time to time fresh food to her young, and remembers very exactly the entrance to her cell, although she has covered it with sand, so as not to be distinguishable from the surrounding surface. Yet M. Fabre found that if he brushed away the earth and the underground passage leading to the nursery, thus exposing the contained larva, the parent insect "was quite at a loss, and did not even recognise her own offspring. It seemed as if she knew the doors, nursery, and the passage, but not her child."

Lastly, instinct is always similarly manifested under similar circumstances by all the individuals of the same species. And, it may be added, these circumstances are always such as have been of frequent occurrence in the life-history of the species.

Now in all these respects instinct differs conspicuously from every other faculty of mind, and especially from reason. Therefore to gather up all these *differentiæ* into one definition, we may say that instinct is the name given to those faculties of mind which are concerned in consciously adaptive action, prior to individual experience, without necessary knowledge of the relation between means employed and ends attained; but similarly performed under similar and frequently recurring circumstances by all the individuals of the same species.

Such being my definition of instinct, I shall now pass on to consider Mr. Darwin's theory of the origin and development of instincts.

Now, to begin with, Mr. Darwin's theory does not, as many suppose that it does, ascribe the origin and development of all instincts to natural selection. This theory does, indeed, suppose that natural selection is an important factor in the process; but it neither supposes that it is the only factor, nor even that in the case of numberless instincts it has had anything at all to do with their formation. Take, for example, the instinct of wildness, or of hereditary fear as directed towards any particular enemy—say man. It has been the experience of travellers who have first visited oceanic islands without human inhabitants, and previously unvisited by man, that the animals are destitute of any fear of man. Under such circumstances the birds have been known to alight on the heads and shoulders of the new-comers, and wolves to come and eat meat held in one hand while a knife was held ready to slay them with the other. But this primitive fearlessness of man gradually passes into an hereditary instinct of wildness, as the special experiences of man's proclivities accumulate; and as this instinct is of too rapid a growth to admit of our attributing it to natural selection (not one per cent. of the animals having been destroyed before the instinct is developed), we can only attribute its growth to the effects of inherited observation. In other words, just as in the lifetime of the individual, adjustive actions which were originally intelligent may by frequent repetition become automatic, so in the lifetime of the species, actions originally intelligent may, by frequent repetition and heredity, so write their effects on the nervous system that the latter is prepared, even before individual experience, to perform adjustive actions mechanically which, in previous generations, were performed intelligently. This mode of origin of instincts has been called by Mr. Lewes the "lapsing of intelligence," and it was fully recognised by Mr. Darwin as a factor in the formation of instinct.

The Darwinian theory of instinct, then, attributes the evolution of instincts to these two causes acting either singly or in combination —natural selection and lapsing intelligence. I shall now proceed to adduce some of the more important facts and considerations which, to the best of my judgment, support this theory, and show it to be by far the most comprehensive and satisfactory explanation of the phenomena which has hitherto been propounded.

That many instincts must have owed their origin and development to natural selection exclusively is, I think, rendered evident by the following general considerations:—

(1) Considering the great importance of instincts to species, we are prepared to expect that they must be in large part subject to the influence of natural selection. (2) Many instinctive actions are performed by animals too low in the scale to admit of our supposing that the adjustments which are now instinctive can ever have been intelligent. (3) Among the higher animals instinctive actions are performed at an age before intelligence, or the power of learning by individual experience, has begun to assert itself. (4) Many instincts, as we now find them, are of a kind which, although performed by intelligent animals at a matured age, yet can obviously never have been originated by intelligent observation. Take, for instance, the instinct

of incubation. It is quite impossible that any animal, prior to individual or ancestral experience, can have kept its eggs warm with the intelligent purpose of developing their contents; so we can only suppose that the incubating instinct began in some such form as we now see it in the spider, where the object of the process is protection, as distinguished from the imparting of heat. But incidental to such protection in the case of a warm-blooded animal is the imparting of heat, and as animals gradually became warm-blooded, no doubt this latter function became of more and more importance to incubation. Consequently, those individuals which most constantly cuddled their eggs would develop most progeny, and so the incubating instinct would be developed by natural selection without there ever having been any intelligence in the matter.

From these four general considerations, therefore, we may conclude (without waiting to give special illustrations of each) that one mode of origin of instincts consists in natural selection, or survival of the fittest, continuously preserving actions which, although never intelligent, yet happen to have been of benefit to the animals which first chanced to perform them. Among animals, both in a state of nature and domestication, we constantly meet with individual peculiarities of disposition and of habit, which in themselves are utterly meaningless, and therefore quite useless. But it is easy to see that if among a number of such meaningless or fortuitous psychological variations, any one arises which happens to be of use, this variation would be seized upon, intensified, and fostered by natural selection, just as in the analogous case of structures. Moreover there is evidence that such fortuitous variations in the psychology of animals (whether useless or accidentally useful) are frequently inherited, so as to become distinctive, not merely of individuals, but of races or strains. Thus, among Mr. Darwin's manuscripts I find a letter from Mr. Thwaites under the date 1860, saying that all his domestic ducks in Ceylon had quite lost their natural instincts with regard to water, which they would never enter unless driven, and that when the young birds were thus compelled to enter the water they had to be quickly taken out again to prevent them from drowning. Mr. Thwaites adds that this peculiarity only occurs in one particular breed. Tumbler pigeons instinctively tumbling, pouter-pigeons instinctively pouting, &c., are further illustrations of the same general fact.

Coming now to instincts developed by lapsing intelligence, I have already alluded to the acquisition of an hereditary fear of man as an instance of this class. Now not only may the hereditary fear of man be thus acquired through the observation of ancestors—and this even to the extent of knowing by instinct what constitutes safe distance from fire-arms; but, conversely, when fully formed it may again be lost by disuse. Thus there is no animal more wild, or difficult to tame, than the young of the wild rabbit; while there is no animal more tame than the young of the domestic rabbit. And the same remark applies, though in a somewhat lesser degree, to the young of the wild and of the domestic duck. For, according to Dr. Rae, "If the eggs of a wild duck are placed with those of a tame duck under a hen to be hatched, the ducklings from the former, on the very day they leave the egg, will immediately endeavour to hide themselves, or take to the water, if there be any water, should anyone approach, whilst the young from the tame duck's eggs will show little or no alarm." Now, as neither rabbits nor ducks are likely to have been selected by man to breed from on account of tameness, we may set down the loss of wildness in the domestic breeds to the uncompounded effects of hereditary memory of man as a harmless animal, just as we attributed the original acquisition of instinctive wildness to the hereditary memory of man as a dangerous animal; in neither case can we suppose that the principle of selection has operated in any considerable degree.

Thus far, for the sake of clearness, I have dealt separately with these two factors in the formation of instinct—natural selection and lapsing intelligence—and have sought to show that either of them working singly is sufficient to develop some instincts. But, no doubt, in the case of most instincts intelligence and natural selection have gone hand in hand, or co-operated, in producing the observed results—natural selection always securing and rendering permanent any advances which intelligence may have made. Thus, to take one case as an illustration. Dr. Rae tells me that the grouse of North America have the curious instinct of burrowing a tunnel just below the surface of the snow. In the end of this tunnel they sleep securely, for when any four-footed enemy approaches the mouth of the tunnel, the bird, in order to escape, has only to fly up through the thin covering of snow. Now in this case the grouse probably began to burrow in the snow for the sake of warmth, or concealment, or both; and, if so, thus far the burrowing was an act of intelligence. But the longer the tunnel the better would it serve in the above-described means of escape; therefore natural selection would tend to preserve the birds which made the longest tunnels, until the utmost benefit that length of tunnel could give had been attained.

And similarly, I believe, all the host of animal instincts may be fully explained by the joint operation of these two causes—intelligent adjustment and survival of the fittest. For now, I may draw attention to another fact which is of great importance, viz. that instincts admit of being modified as modifying circumstances may require. In other words, instincts are not rigidly fixed, but are plastic, and their plasticity renders them capable of improvement or of alteration, according as intelligent observation requires. The assistance which is thus rendered by intelligence to natural selection must obviously be very great, for under any change in the surrounding conditions of life which calls for a corresponding change in the ancestral instincts of the animal, natural selection is not left to wait, as it were, for the required variations to arise fortuitously; but is from the first furnished by the intelligence of the animal with the particular variations which are needed.

In order to demonstrate this principle of the variation of instinct under the guidance of intelligence, I may here introduce a few examples.

Huber observes, "How ductile is the instinct of bees, and how readily it adapts itself to the place, the circumstances, and the needs of the community." Thus, by means of contrivances which I need not here explain, he forced the bees either to cease building combs, or to change their instinctive mode of building from above downwards, to building in the reverse direction, and also horizontally. The bees in each case changed their mode of building accordingly. Again, an irregular piece of comb, when placed by Huber on a smooth table, tottered so much that the bumble bees could not work on so unsteady a basis. To prevent the tottering, two or three bees held the comb by fixing their front feet on the table, and their hind feet on the comb. This they continued to do, relieving guard, for three days, until they had built supporting pillars of wax. Some other bumble bees, when shut up and so prevented from getting moss wherewith to cover their nests, tore threads from a piece of cloth, and "carded them with their feet into a fretted mass," which they used as moss. Lastly, Andrew Knight observed that his bees availed themselves of a kind of cement made of rosin and turpentine, with which he had covered some decorticated trees—using this ready-made material instead of their own propolis, the manufacture of which they discontinued; and more recently it has been observed that bees, "instead of searching for pollen, will gladly avail themselves of a very different substance, namely, oatmeal." Now in all these cases it is evident that if, from any change of environment, such accidental conditions were to occur in a state of nature, the bees would be ready at any time to meet them by intelligent adjustment, which, if continued sufficiently long and aided by selection, would pass into true instincts of building combs in new directions, of supporting combs during their construction, of carding threads of cloth, of substituting cement for propolis and oatmeal for pollen.

Turning to higher animals, Andrew Knight tells us of a bird which, having built her nest upon a forcing-house, ceased to visit it during the day when the heat of the house was sufficient to incubate the eggs; but always returned to sit upon the eggs at night when the temperature of the house fell. Again, thread and worsted are now habitually used by sundry species of birds in building their nests, instead of wool and horse-hair, which in turn were no doubt originally substitutes for vegetable fibres and grasses. This is especially noticeable in the case of the tailor-bird, which finds thread the best material wherewith to sew. The common house-sparrow furnishes another instance of intelligent adaptation of nest-building to circumstances, for in trees it builds a domed nest (presumably, therefore, the ancestral type), but in towns avails itself by preference of sheltered holes in buildings, where it can afford to save time and trouble by constructing a loosely-formed nest. Moreover, the chimney- and house-swallows have similarly changed their instincts of nidification, and in America this change has taken place within the last two or three hundred years. Indeed, according to Captain Elliott Coues, all the species of swallow on that continent (with one possible exception) have thus modified the sites and structures of their nests in accordance with the novel facilities afforded by the settlement of the country.

Another instructive case of an intelligent change of instinct in connection with nest-building is given from a letter by Mr. Haust, dated New Zealand, 1862, which I find among Mr. Darwin's manuscripts. Mr. Haust says that the Paradise duck, which naturally or usually builds its nest along the rivers on the ground, has been observed by him on the east of the island, when disturbed in their nests upon the ground, to build "new ones on the tops of high trees, afterwards bringing their young ones down on their backs to the water;" and exactly the same thing has been recorded by another observer of the wild ducks of Guiana. Now if intelligent adjustment to peculiar circumstances is thus adequate, not only to make a whole breed or species of bird transport their young upon their backs—or, as in the case of the woodcock, between their legs—but even to make web-footed water-fowl build their nests in high trees, I think we can have no doubt that if the need of such adjustment were of sufficiently long continuance, the intelligence which leads to it would eventually produce a new and remarkable modification of their ancestral instinct of nest-building.

Turning now from the instinct of nidification to that of incubation, I may give one example to show the plasticity of the instinct in relation to the observed requirements of progeny. Several years ago I placed in the nest of a sitting Brahma hen, four newly-born ferrets. She took to them almost immediately, and remained with them for rather more than a fortnight, when I made a separation. During the whole of the time the hen had to sit upon the nest, for the young ferrets were not able to follow her about, as young chickens would have done. The hen was very much puzzled by the lethargy of her offspring, and two or three times a day she used to fly off the nest calling on her brood to follow; but, on hearing their cries of distress from cold, she always returned immediately, and sat with patience for six or seven hours more. I found that it only took the hen one day to learn the meaning of these cries of distress; for after the first day she would always run in an agitated manner to any place where I concealed the ferrets, provided that this place was not too far away from the nest to prevent her from hearing their cries. Yet I do not think it would be possible to imagine a greater contrast between two cries than the shrill piping note of a young chi
the hoarse growling noise of a young ferret. At times
to fly off the nest with a loud scream, which was doubt
unaccustomed sensation of being nipped by the youn
search for the traditional source of mammalian
further worthy of remark that the hen showed

the ferrets were taken from the nest to be fed, that I adopted the plan of giving them the milk in their nest, and with this arrangement the hen seemed quite satisfied; at any rate she used to chuck when she saw the milk coming, and surveyed the feeding with evident satisfaction.

Thus we see that even the oldest and most important of instincts in bees and birds admit of being greatly modified, both in the individual and in the race, by intelligent adaptation to changed conditions of life; and therefore we can scarcely doubt that the principle of lapsing intelligence must be of much assistance to that of natural selection in the origination and development of instinct.

I shall now turn to another branch of the subject. From the nature of the case it is not to be expected that we should obtain a great variety of instances among wild animals of new instincts acquired under human observation, seeing that the conditions of their life, as a rule, remain pretty uniform for any periods over which human observation can extend. But from a time before the beginning of history, mankind, in the practice of domesticating animals, has been making what we may now deem a gigantic experiment upon the topic before us.

The influences of domestication upon the psychology of animals may be broadly considered as both negative and positive—negative in the obliteration of natural instincts; positive in the creation of artificial instincts. I shall consider these two branches separately, and here I may again revert to the obliteration of natural wildness. We all know that the horse is an easily breakable animal, but his nearest allies in a state of nature, the zebra and the quagga, are the most obstinately unbreakable of animals. Similar remarks apply to the natural wildness of all wild species of kine, as contrasted with the innate tameness of our domesticated breeds. Consider again the case of the cat. The domesticated animal is sufficiently tame, even from kittenhood; whereas its nearest cousin in a state of nature, the wild cat, is perhaps of all animals the most untameable. But of course it is in the case of the dog that we meet with the strongest evidence on this point. The most general and characteristic features in the psychology of all the domesticated varieties are faithfulness, docility, and sense of dependence upon a master; whereas the most usual and characteristic features in the psychology of all the wild species are fierceness, treachery, and self-reliance. But, not further to pursue the negative side of this subject, let us now turn to the positive, or to the power which man has shown himself to possess of implanting new instincts in the mental constitution of animals. For the sake of brevity I shall here confine myself to the most conspicuous instance, which is of course furnished by the dog, seeing that the dog has always been selected and trained with more or less express reference to his mental qualities. And here I may observe that in the process of modifying psychology by domestication exactly the same principles have been brought into operation as those to which we attribute the modification of instincts in general; for the processes of artificial selection and training in successive generations are precisely analogous to the processes of natural selection and lapsing of intelligence in a state of nature.

Touching what Mr. Darwin calls the artificial instincts of the dog, I may first mention those which he has himself dilated upon—I mean the instincts of pointing, retrieving, and sheep-tending; but as Mr. Darwin has already fully treated of these instincts, I need not go over the ground which he has traversed, and so shall confine myself to the consideration of another artificial instinct, which, although not mentioned by him, seems to me of no less significance—I mean the instinct of guarding property. This is a purely artificial instinct, created by man expressly for his own purposes: and it is now so strongly ingrained in the intelligence of the dog that it is unusual to find any individual animal in which it is wholly absent. Thus, we all know, that without any training a dog will allow a stranger to pass by his master's gate without molestation, but that as soon as the stranger passes within the gate, and so trespasses upon what the dog knows to be his master's territory, the animal immediately begins to bark in order to give his master notice of the invasion. And this leads me to observe that barking is in itself an artificial instinct, developed, I believe, as an offshoot from the more general instinct of guarding property. None of the wild species of dog are known to bark, and therefore we must conclude that barking is an artificial instinct, acquired by the domestic dog for the purpose of notifying to his master the presence of thieves or enemies. I may further observe that this instinct of guarding property extends to the formation of an instinctive idea on the part of the animal, of itself as constituting part of that property. If, for instance, a friend gives you temporary charge of his dog, even although the dog may never have seen you before, observing that you are his master's friend and that his master intends you to take charge of him, he immediately transfers his allegiance from his master to you, as to a deputed owner, and will then follow you through any number of crowded streets with the utmost confidence. Thus, whether we look to the negative or to the positive influences of domestication upon the psychology of the dog, we must conclude that a change has been wrought, so profound that the whole mental constitution of the animal now presents a more express reference to the needs of another, and his enslaving animal, than it does to his own. Indeed, we may say that there is no one feature in the whole psychology of the dog which has been left unaltered by the influence of man, excepting only those instincts which, being neither useful nor harmful to man, have never been subject to his operation—such, for instance, as the instinct of burying food, turning round to make a bed before lying down, &c.

I will now turn to another branch of the subject, and one which, although in my opinion of the greatest importance, has never before been alluded to; I mean the local and specific variations of instinct. By a local variation of instinct, I mean a variation presented by a species in a state of nature over some particular area of geographical distribution. It is easy to see the importance of such local variations of instinct as evidence of the transmutation of instinct, if we reflect that such a local variation is obviously on its way to becoming a new instinct. For example, the beavers in California have ceased to make dams, the hyænas in South Africa have ceased to make burrows, and there is a squirrel in the neighbourhood of Mount Airy which has developed carnivorous tastes—running about the trees, not to search for nuts, but to search for birds, the blood of which it sucks. In Obinitahi there is a mountain parrot which before the settlement of the place was a honey-eater, but when sheep were introduced the birds found that mutton was more palatable to them than honey, and quickly abandoned their ancestral habits, exchanging their simple tastes of honey-eaters for the savageness of tearers of flesh. For the birds come in flocks, single out a sheep, tear out the wool, and when the sheep, exhausted by running about, falls upon its side, they bore into its abdominal cavity to get at the fat which surrounds the kidneys.

These, I think, are sufficient instances to show what I mean by local variations of instinct. Turning now to the specific variations, I think they constitute even stronger evidence of the transmutation of instinct; for where we find an instinct peculiar to a species, or not occurring in any other species of the genus, we have the strongest possible evidence of that particular instinct having been specially developed in that particular species. And this evidence is of particular cogency when, as sometimes happens, the change of instinct is associated with structures pointing to the state of the instincts before the change. Thus, for example, the dipper belongs to a non-aquatic family of birds, but has developed the instinct, peculiar to its species, of diving under water and running along the bottoms of streams. The species, however, has not had time, since the acquisition of this instinct, to develop any of the structures which in all aquatic families of birds are correlated with their aquatic instincts, such as webbed feet, &c. That is to say, the bird retains all its structural affinities, while departing from the family type as regards its instincts. A precisely converse case occurs in certain species of birds belonging to families which are aquatic in their affinities, these species, however, having lost their aquatic instincts. Such is the case, for example, with the upland geese. These are true geese in all their affinities, retaining the webbed feet, and all the structures suited to the display of aquatic instincts; yet they never visit the water. Similarly, there are species of parrots and tree frogs, which, while still retaining the structures adapted to climbing trees, have entirely lost their arboreal habits. Now, short of actual historical or palæontological information—which of course in the case of instincts is unattainable, seeing that instincts, unlike structures, never occur in a fossil state—short, I say, of actual historical or palæontological information, we could have no stronger testimony to the fact of transmutation of instincts than is furnished by such cases, wherein a particular species while departing from the instinctive habits of its nearest allies, still retains the structures which are only suited to the instincts now obsolete.

This last head of evidence—that, namely, as to local and specific variations of instincts—differs in one important respect from all the other heads of evidence which I have previously adduced. For while these other heads of evidence had reference to the theory concerning the *causes* of transmutation, this head of evidence has reference to the *fact* of transmutation. Whatever, therefore, we may think concerning the evidence of the causes, it is quite distinct from that on which I now rely as conclusive proof of the fact.

I shall now, for the sake of fairness, briefly allude to the more important cases of special difficulty which lie against Mr. Darwin's theory of the origin and development of instincts. For the sake of brevity, however, I shall not allude to those cases of special difficulty which he has himself treated in the 'Origin of Species,' but shall confine myself to considering the other and most formidable cases which, after surveying all the known instincts presented by animals, I have felt to be such.

First, we have the alleged instinct of the scorpion committing suicide when surrounded by fire. This instinct, if it really exists, would no doubt present a difficulty, because it is clearly an instinct which, being not only of no use, but actually detrimental both to the individual and the species, could never have been developed either by natural selection or by lapsing intelligence. I may, however, dismiss this case with a mere mention, because as yet the evidence of the fact is not sufficiently precise to admit of our definitely accepting it as a fact.

There can be no such doubt, however, attaching to another instinct largely prevalent among insects, and which is unquestionably detrimental, both to the individual and to the species. I allude to the instinct of flying through flame. This is unquestionably a true instinct, because it is manifested by all individuals of the same species. How then are we to explain its occurrence? I think we may do so by considering, in the first place, that flame is not a sufficiently common object in nature to lead to any express instinct for its avoidance; and in the next place by considering that insects unquestionably manifest a disposition to approach and examine shining objects. Whether this disposition is due to mere curiosity, or to a desire to ascertain if the shining objects will, like flowers, yield them food, is a question which need not here concern us. We have merely to deal with the fact that such a general disposition is displayed. Taking then this fact, in connection with the fact that flame is not a sufficiently common object in nature to lead to any instinct expressly directed against its avoidance, it seems to me that the difficulty we are considering is a difficulty no longer.

The shamming-dead of insects appears at first sight a formidable difficulty, because it is impossible to understand how any insect can have acquired the idea of death or of its intentional simulation. This difficulty occurred to Mr. Darwin thirty or forty years ago, and among his manuscripts I find some very interesting notes of experiments upon the subject. He procured a number of insects which exhibited the instinct, and carefully noted the attitude in which they feigned death. Some of these insects he then killed, and he found that in no case did the attitude in which they feigned death resemble the attitude in which they really died. Consequently we must conclude that all the instinct amounts to is that of remaining motionless, and therefore inconspicuous, in the presence of danger; and there is no more difficulty in understanding how such an instinct as this should be developed by natural selection in an animal which has no great powers of locomotion, than there is in understanding how the instinct to run away from danger should be developed in another animal with powers of rapid locomotion. The case, however, is not, I think, quite so easy to understand in the feigning death of higher animals. From the evidence which I have I find it almost impossible to doubt that certain birds, foxes, wolves, and monkeys, not to mention some other and more doubtful cases, exhibit the peculiarity of appearing dead when captured by man. As all these animals are highly locomotive, we cannot here attribute the fact to protective causes. Moreover, in these animals this behaviour is not truly instinctive, inasmuch as it is not presented by all, or even most individuals. As yet, however, observation of the facts is insufficient to furnish any data as to their explanation, although I may remark that possibly they may be due to the occurrence of the mesmeric or hypnotic state, which we know from recent researches may be induced in animals under the influence of forcible manipulation.

The instinct of feigning injury by certain birds presents a peculiar difficulty. As we all know, partridges, ducks, and plovers, when they have a brood of young ones, and are alarmed by the approach of a carnivorous quadruped, such as a dog, will pretend to be wounded, flapping along the ground with an apparently broken wing in order to induce the four-footed enemy to follow, and thus to give time for the young brood to disperse and hide themselves. The difficulty here, of course, is to understand how the birds can have acquired the idea of pretending to have a broken wing, for the occasions must be very rare on which any bird has seen a companion thus wounded followed by a carnivorous quadruped; and even if such observations on their part were of frequent occurrence, it would be difficult to accredit the animals with so high a degree of reasoning power as would be required for them intentionally to imitate such movements. When I consulted Mr. Darwin with reference to this difficulty, he gave me a provisional hypothesis by which it appeared to him that it might be met. He said that anyone might observe, when a hen has a brood of young chickens and is threatened by a dog, that she will alternately rush at the dog and back again to the chickens. Now if we could suppose that under these circumstances the mother bird is sufficiently intelligent to observe that when she runs away from the dog, she is followed by the dog, it is not impossible that the maternal instinct might induce her to run away from a brood in order to lead the dog away from it. If this happened in any cases, natural selection would tend to preserve those mother birds which adopted this device. I give this explanation as the only one which either Mr. Darwin or myself has been able to suggest. It will be observed, however, that it is unsatisfactory, inasmuch as it fails to account for the most peculiar feature of the instinct—I mean the trailing of the apparently wounded wing.

The instinct of migration furnishes another case of special difficulty, but as I have no space to dwell upon the sundry questions which it presents for solution, I shall now pass on to the last of the special difficulties which most urgently call for consideration. The case to which I refer deserves, I think, to be regarded as the most extraordinary instinct in the world. There is a species of wasp-like insect, called the Sphex. This insect lays its eggs in a hole excavated in the ground. It then flies away and finds a spider, which it stings in the main nerve-centre of the animal. This has the effect of paralysing the spider without killing it. The sphex then carries the now motionless spider to its nursery, and buries it with the eggs. When the eggs hatch out the grubs feed on the paralysed prey, which is then still alive and therefore quite fresh, although it has never been able to move since the time when it was buried. Of course the difficulty here is to understand how the sphex insect can have acquired so much anatomical and physiological knowledge concerning its prey as the facts imply. We might indeed suppose, as I in the first instance was led to suppose, that the sting of the sphex and the nerve-centre of the spider being both organs situated on the median line of their respective possessors, the striking of the nerve-centre by the sting might in the first instance have been thus accidentally favoured, and so have supplied a basis from which natural selection could work to the perfecting of an instinct always to sting in one particular spot. But more recently the French entomologist, M. Fabre, who first noticed these facts with reference to the stinging of the spider, has observed another species of sphex which preys upon the grasshopper, and as the nervous system of a grasshopper is more elongated than the nervous system of a spider, the sphex in this case has to sting its prey in three successive nerve-centres in order to induce paralysis. Again, still more recently, M. Fabre has found another species of sphex, which preys upon a caterpillar, and in this case the animal has to sting its victim in nine successive nerve-centres. On my consulting Mr. Darwin in reference to these astonishing facts, he wrote me the following letter:—

"I have been thinking about Pompilius and its allies. Please take the trouble to read on perforation of the corolla, by Bees, p. 425, of my 'Cross-fertilisation,' to end of chapter. Bees shows so much *intelligence* in their acts, that it seems not improbable to me that the progenitors of Pompilius originally stung caterpillars and spiders, &c., in any part of their bodies, and then observed by their intelligence that if they stung them in one particular place, as between certain segments on the lower side, their prey was at once paralysed. It does not seem to me at all incredible that this action should then become instinctive, i. e. memory transmitted from one generation to another. It does not seem necessary to suppose that, when Pompilius stung its prey in the ganglion it intended, or knew, that the prey would keep long alive. The development of the larvæ may have been subsequently modified in relation to their half-dead, instead of wholly dead prey; supposing that the prey was at first quite killed, which would have required much stinging. Turn over this in your mind, &c."

I confess that this explanation does not appear to me altogether satisfactory, although it is no doubt the best explanation that can be furnished on the lines of Mr. Darwin's theory.

In the brief time at my disposal, I have endeavoured to give an outline sketch of the main features of the evidence which tends to show that animal instincts have been slowly evolved under the influence of natural causes, the discovery of which we owe to the genius of Darwin. And, following the example which he has set, I shall conclude by briefly glancing at a topic of wider interest and more general importance. The great chapter on Instinct in the 'Origin of Species' is brought to a close in the following words:—

"Finally it may not be a logical deduction, but to my imagination it is far more satisfactory to look at such instincts as the young cuckoo ejecting its foster-brothers, ants making slaves, the larvæ of ichneumonidæ feeding within the live bodies of caterpillars, not as specially endowed or created instincts, but as small consequences of one general law leading to the advancement of all organic beings, namely, multiply, vary, let the strongest live, and the weakest die."

This law may seem to some, as it has seemed to me, a hard one —hard, I mean as an answer to the question which most of us must at some time and in some shape have had faith enough to ask, "Shall not the Judge of all the earth do right?" For this is a law, rigorous and universal, that the race shall always be to the swift, the battle without fail to the strong; and in announcing it the voice of science has proclaimed a strangely new beatitude—Blessed are the fit, for they shall inherit the earth. Surely these are hard sayings, for in the order of nature they constitute might the only right. But if we are thus led to feel a sort of moral repugnance to Darwinian teaching, let us conclude by looking at this matter a little more closely, and in the light that Darwin himself has flashed upon it in the short passage which I have quoted.

Eighteen centuries before the publication of this book—the 'Origin of Species'—one of the founders of Christianity had said, in words as strong as any that have been used by the Schopenhauers and Hartmanns of to-day, "the whole creation groaneth in pain and travail." Therefore we did not need a Darwin to show us this terrible truth; but we did need a Darwin to show us that out of all the evil which we see, at least so much of good as we have known has come; that if this is a world of pain and sorrow, hunger, strife, and death, at least the suffering has not been altogether profitless; that whatever may be "the far-off divine event to which the whole creation moves," the whole creation, in all its pain and in all its travail, is certainly moving, and this in a direction which makes, if not for "righteousness," at all events for improvement. No doubt the origin of evil has proved a more difficult problem to solve than the origin of species; but, thus viewed, I think that the Darwinian doctrine deserves to be regarded as in some measure a mitigation of the difficulty: certainly in no case an aggravation of it. I do not deny that an immense residuum of difficulty remains, seeing that, so far as we can judge, the means employed certainly do not appear to be justified by the ends attained. But even here we ought not to lose sight of the possibility that, if we could see deeper into the mystery of things, we might find some further justification of the evil, as unsuspected as was that which, as it seems to me, Darwin has brought to light. It is not in itself impossible—perhaps it is not even improbable—that the higher instincts of man may be pointing with as true an aim as those lower instincts of the brutes which we have been contemplating. And, even if the theory of evolution were ever to succeed in furnishing as satisfactory an explanation of the natural development of the former as it has of the natural development of the latter, I think that the truest exponent of the meaning—as distinguished from the causation—of these higher instincts would still be, not the man of science, but the poet. Here, therefore, it seems to me, that men of science ought to leave the question of pain in Nature to be answered, so far as it can be answered, by the general voice of that humanity which we all share, and which is able to acknowledge that at least its own allotment of suffering is not an unmitigated evil.

"For clouds of sorrow deepness lend
To change joy's early rays,
And manhood's eyes alone can send
A grief-ennobled gaze;

"While to that gaze alone expand
Those skies of fullest thought,
Beneath whose star-lit vault we stand,
Lone, wondering, and untaught."

"We look before and after,
And pine for what is not,
Our sincerest laughter
With some pain is fraught."

Yet still,—

"Our sweetest songs are those that tell of saddest thought."

[G. J. R.]

WEEKLY EVENING MEETING,

Friday, May 9, 1884.

THE DUKE OF NORTHUMBERLAND, D.C.L. LL.D. President,
in the Chair.

PROFESSOR W. ROBERTSON SMITH, M.A. LL.D.

Mohammedan Mahdis.

1. The name Mahdī. Al-Mahdī is not a corruption of Al-muhdi, and does not mean "the spiritual guide," as has been lately asserted by scholars both in England and in the East, but as can be proved by the metre of poems in which it occurs is a *passive* form (Al-Mahdîyu, "the rightly-guided one"). The idea of the Mahdī is therefore that of a chief of Islām who is himself infallibly guided by God.

2. This idea does not belong to the original doctrine of Islām, which makes Mohammed the seal of the prophets and regards the Koran as all-sufficient for the future. Source of Mohammed's theory of revelation by a *book;* its inadequacy to sustain and inspire the Moslems after his death except in their first career of victory; the empire of the Caliphs did not realise the ideal of a theocracy guided wholly by the Koran even for the Arabs and much less for the Persians.

3. Rise of Messianic ideas; points of resemblance to and distinction from such ideas in Judaism. The Messianic idea takes shape among the Persians in conformity with their high-flown notions of Kingship as almost divine. The tragedy of Ali and his family leads the Shia to accept his family as legitimate and expect the golden age from its restoration.

4. The doctrine of the hidden Imām arises, on the failure of successive attempts at religious revolution in favour of the Alidae. In the darkest times God's sovereignty on earth is represented by a *hidden* leader who in due time will appear in victory. The hidden Imām is the Mahdī, and both names appear together for the first time in connection with Mohammed ibn al Hanafīya.

5. The Shiite doctrine of the Imām modified so as to become influential outside the Shia circle: the Ismailis. Abdullah al Kaddah and his secret society; the Carmathians; the Fatimites.

6. Ultimately the doctrine of the Mahdi becomes a stereotyped element in all popular risings within Islam against oppression, and is not limited to the Shia. A typical example is found in Ibn Tumert the Almohade Mahdi. His history proves him to have been an impostor, not an enthusiast, and this is almost always found to be the case with political Mahdis. Of unpolitical and unselfish Mahdis the chief type is the Persian religious teacher Bab.

WEEKLY EVENING MEETING,

Friday, June 13, 1884.

WARREN DE LA RUE, Esq. M.A. D.C.L. F.R.S. Manager and Vice-President, in the Chair.

PROFESSOR JAMES DEWAR, M.A. F.R.S. *M.R.I.*

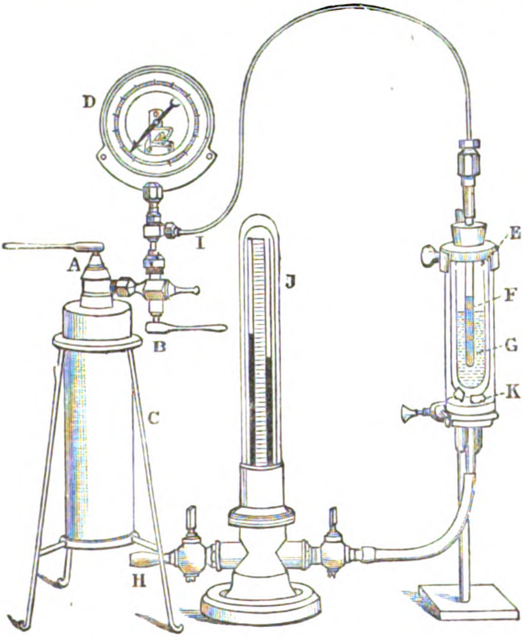
Researches on Liquefied Gases.[*]

THE two Russian chemists, MM. Wroblewski and Olzewski, who have recently made such a splendid success in the production and maintenance of low temperature, have used in their researches an enlarged form of the well-known Cailletet apparatus; but for the purposes of lecture demonstration, which necessarily involves the projection on a screen of the actions taking place, the apparatus represented in the annexed woodcut is more readily and quickly handled, and enables comparatively large quantities of liquid oxygen to be produced. The arrangements will be at once understood on looking at the figure, which is taken from a photograph. The oxygen- or air-reservoir, C, is made of iron; it contains gas compressed for convenience to 150 atmospheres. A is the stopcock for regulating the pressure of the gas in the glass tube F, and D is the pressure-manometer, the fine copper tube which connects the gas-reservoir and the glass tube, F, being shown at I. The air-pump gauge is marked J, the tube leading to the double oscillating Bianchi being attached at H. The glass test-tube G, which contains the liquid ethylene, solid carbonic acid, or liquid nitrous oxide, which is to be boiled *in vacuo*, is placed in the middle of a larger tube. It has holes, shown at E, in the upper part, so that the cool vapours in their course to the air-pump are forced to pass round the outside of the vessel and help to guard it from external radiation. The lower part of the outer cylinder is covered with pieces of chloride of calcium, shown at K. If a thermometer is used and a continuous supply of ethylene maintained, the indiarubber cork through which the tube F passes has two additional apertures for the purpose of inserting the respective tubes. When the pump has reduced the pressure to 25 mm., the ethylene has a temperature of about −140° C.; a pressure of between 20 and 30 atmospheres is then sufficient to produce liquid oxygen in the tube F. The tube F is 5 mm. in diameter and about 3 mm. thick in the walls, and when filled with fluid oxygen (for projection) holds at least 1·5 cubic centim. With such a quantity of fluid oxygen it is easy to show its ebullition at ordinary pressures, and by means of a thermo-junction to demonstrate the great reduction of temperature which is attendant on its change of state at atmospheric pressure.

---

[*] See Professor Dewar's discourse on the Liquefaction of Gases.—'Proceedings' of the Royal Institution, vol. viii. p. 657.

Provided a supply of liquid ethylene can be had, there is no difficulty in repeating all the experiments of the Russian observers; but as this gas is troublesome to make in quantity, and cannot be bought like carbonic acid or nitrous oxide, such experiments necessitate a considerable sacrifice of time. It was therefore with considerable satisfaction that I observed the production of liquid oxygen by the use of solid carbonic acid, or preferably liquid nitrous oxide. When these substances are employed and the pressure is reduced to about 25 mm., the temperature of −115° C. may be taken as that of the carbonic acid, and −125° C. as that of the nitrous oxide. As the critical point of oxygen, according to the Russian observers, is about −113° C., both these cooling agents may be said to lower the temperature sufficiently to produce liquid oxygen, provided a pressure of the gas above the critical pressure, which is 50 atmospheres, is at command. In any case, however, the temperature is near that of the critical point; and as it is difficult to maintain the pressure below about an inch of mercury, the temperature is apt to be rather above



the respective temperatures of −115° C. and −125° C. In order to get liquefaction conveniently with either of these agents, it is necessary to work at a pressure of oxygen gas from 80 to 100 atmospheres, and to have the means of producing a sudden expansion when the compressed gas is cooled to the above-mentioned temperatures. This is brought about by the use of an additional stopcock, represented in the figure at B. During the expansion the stopcock at A is closed and the pressure-manometer carefully observed. No doubt liquid nitrous oxide is the most convenient substance to use as a cooling agent; but as it is apt to get superheated during the reduction of pressure and boil over with explosive bursts of vapour, it is well to collect the fluid in a small flask of about 250 cub. centim. capacity, and to change it into the solid state by connecting the flask with the air-pump, and then to use the substance in this form. The addition of alcohol or ether to the solid nitrous oxide makes the body more transparent, and thereby favours the observations.

It is evident that this apparatus enables the observer to determine the density of the fluid gases condensed in the tube F; since he has only to measure the volume of fluid in F, and to collect, by means of the stopcock B, the whole volume of gas given by the fluid and condensed vapour, which gives an accurate determination of the total weight of substance distributed between fluid and vapour in the whole apparatus. The amount of substance which is required to produce the vapour is easily found by observing the vapour-pressure of the liquid gas before expanding it into gas for the volume measurement; and while keeping shut the stopcock B, by opening A suddenly until this pressure is just reached and then instantly shutting off the receiver. If this volume of gas is now measured by opening B as before, the difference between the two volumes thus collected will correspond to the real weight of substance in the liquid state. A rough experiment with oxygen near the critical point gave the density 0·65.

As to the most convenient substance for use as a cooling agent, I am still of opinion that marsh-gas would be the best; and I may take the opportunity of pointing out that the employment of this body was suggested by me in a communication made to the Chemical Section of the British Association in 1883. The following extract from 'Nature,' of October 4, 1883, will prove that my experiments with liquid marsh-gas were made a year in advance of those made recently by M. Cailletet * and M. Wroblewski †:—

"Professor Dewar pointed out an important relation between the critical temperatures and pressures of volatile liquids and their molecular volumes. The ratio of the critical temperature to the

---

* "Sur l'emploi du Formène pour la production des très basses températures," *Comptes Rendus*, June 30, 1884.

† "Sur les propriétés du gaz des marais liquide, et sur son emploi comme réfrigérant," *Comptes Rendus*, July 21, 1884.

critical pressure is proportional to the molecular volume, so that the determination of the critical temperature and pressure of a substance gives us a perfectly independent measure of the molecular volumes. Prof. Dewar pointed out the great advantage of employing a liquid of low critical temperature and pressure such as liquid marsh-gas for producing exceedingly low temperature. He hoped to be able to approach the absolute zero by the evaporation of liquefied marsh-gas whose critical temperature was less than −100° C., and whose critical pressure was only 39 atmospheres."

I ought to mention that the marsh-gas used in my experiments was made by the action of water on zinc methyl, and was therefore very pure, and that the observed critical pressure was not 39 atmospheres, but 47·6. The following table gives the values of the ratio of the absolute critical temperature to the critical pressure in the case of a number of substances. The values for ammonia, sulphuretted hydrogen, cyanogen, marsh-gas, and hydride of ethyl are new.

		T. Critical temperature.	P. Critical pressure.	$\frac{T}{P}$.
Chlorine	Cl_2	141°·0	83·9	5·0
Hydrochloric acid	HCl	52·3	86·0	3·7
Oxygen	O_2	−113·0	50·0	3·2
Water	H_2O	370·0	195·5	3·3
Nitrogen	N_2	−146·0	35·0	3·6
Hydrogen sulphide	H_2S	100·2	92·0	4·0
Ammonia	H_3N	130·0	115·0	3·5
Diethylamine	$(C_2H_5)_2HN$	220·0	38·7	15·4
Nitrous oxide	N_2O	35·4	75·0	4·1
Sulphurous acid	SO_2	155·4	78·9	5·4
Marsh-gas	CH_4	− 99·5	50·0	3·5
Acetylene	C_2H_2	37·0	68·0	4·5
Ethylene	C_2H_4	10·1	51·0	5·5
Ethyl hydride	C_2H_6	35·0	45·2	6·8
Amylene	C_5H_{10}	191·6	33·9	13·7
Benzol	C_6H_6	291·7	60·4	9·3
Chloroform	$CHCl_3$	268·0	54·9	9·9
Carbon chloride	CCl_4	282·0	57·6	9·6
Carbonic acid	CO_2	31·9	77·0	4·0
Bisulphide of carbon	CS_2	277·7	78·1	7·0
Cyanogen	C_2N_2	124·0	61·7	6·4

A glance at the last column of the table shows that a large number of substances have at their respective critical temperatures simple volume relations. Thus hydrochloric acid, water, ammonia, and marsh-gas, the four chemical substances from which the great majority of chemical compounds may be derived by processes of substitution, have nearly the same volume; while the more complex derivatives show an increased volume which bears a simple ratio to that of the typical body. As the critical pressures are not known with any great accuracy at present, it would be useless to discuss the results with any severity. All that can be inferred is that the subject is worthy of further investigation and promises important generalisation. Sarrau (*Compt. Rend.* 1882) deduced the critical temperatures and pressures of hydrogen, oxygen, and nitrogen by the application of Clausius' formula to the experiments of Amagat; and it is interesting to compare his results with the experimental values.

Sarrau's Calculated Values.

	T. Critical temperature.	P. Critical pressure.	$\frac{T}{P}$.
Hydrogen	−174°	98·9	1·0
Oxygen	−105·4	48·7	3·4
Nitrogen	−124	42·1	3·5

It will be observed that the calculated critical temperatures of oxygen and nitrogen are remarkably near the truth, being respectively 8° and 22° too high. On the other hand, the values of the ratios of the calculated critical temperatures and pressures are almost identical with those obtained by direct experiment. The only peculiarity to be noted is in the case of hydrogen, which has such a high critical pressure, and therefore leads to a remarkably small molecular volume at the critical point. If the values of the T÷P ratio be taken as proportional to the molecular volumes, then it is easy to infer the densities of the fluids at their respective critical temperatures, provided the density of one standard substance is known by experiment. The simple formula thus stated is

$$\frac{S'}{S} = \Psi \frac{W'}{W} \Psi = \frac{V}{W},$$

where S and S′ are the specific gravities of two bodies, W and W′ their molecular weights, and V and V′ their molecular volumes. It will be convenient to take the density of carbonic acid at the critical point as the standard density to which the others can be referred. The density of carbonic acid under such conditions may be taken as 0·65. Calculating with the above formula, the density of acetylene would be 0·32, whereas the experimental number of Ansdell is 0·36. In the same way the density of hydrochloric acid is found to be 0·6, the true value being 0·61. The density of oxygen would be 0·63, and that of nitrogen 0·45. The calculated density of hydrogen at its critical point would be 0·12, if we assume the correctness of Sarrau's values for the critical temperature and pressure. We may compare these values with the numbers obtained by Cailletet and Hautefeuille for the densities of oxygen, nitrogen, and hydrogen from their experiments on the density of liquid carbonic acid obtained from mixtures of this body with these gases. At the temperature of 0° C. the experiments found for oxygen, nitrogen, and hydrogen the respective values of 0·65, 0·37, and 0·025. It seems that the calculated values for oxygen and nitrogen are not very far wrong; but hydrogen is clearly incorrect. The explanation of this anomaly is probably to be found in the fact that the calculated molecular volume of hydrogen is wrong, and that instead of being unity on our scale it ought to be 3·5 like oxygen and nitrogen. In fact, the chemist would infer that, as the difference in the complexity of the molecular structure of hydrochloric acid, water, ammonia, and marsh-gas does not affect the molecular volume under the conditions we are discussing, in all probability the value for hydrogen would be identical with that of the above-mentioned bodies. If we adopt this view and change the value of the $T_c \div P_c$ to 3·5, then the density of the fluid would become 0·034, which is in accordance with the experimental number of Cailletet and Hautefeuille. An accurate determination of the critical temperature and pressure of hydrogen, for which, judging from the success of the experiments of M. Olzewski, chemists will not have to wait long, will thus be of great interest.

[J. D.]

GENERAL MONTHLY MEETING,

Monday, July 7, 1884.

The Hon. Sir William R. Grove, M.A. D.C.L. LL.D. F.R.S.
Manager and Vice-President, in the Chair.

James Wimnshurst, Esq.

was elected a Member of the Royal Institution.

The decease of The Right Hon. Lord Claud Hamilton, a Manager, was announced from the Chair.

The Managers, at their Meeting this day, appointed Professor Arthur Gamgee, M.D. F.R.S. Fullerian Professor of Physiology for three years.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Academy of Natural Sciences, Philadelphia—Proceedings, 1884, Part 1. 8vo. 1884.
Antiquaries, Society of—Proceedings, Vol. IX. No. 3. 8vo. 1883.
Asiatic Society of Bengal—Proceedings, No. 2. 8vo. 1884.
Journal, Vol. LIII. Part I. No. 1. 8vo. 1884.
Astronomical Society, Royal—Monthly Notices, Vol. XLIV. No. 7. 8vo. 1884.
Bankers, Institute of—Journal, Vol. V. Part 6. 8vo. 1884.
British Architects, Royal Institute of—Proceedings, 1883-4, No. 15. 4to.
Brymner, Douglas, Esq. (the Author)—Report on Canadian Archives, 1883. 8vo. Ottawa, 1884.
Chemical Society—Journal for June, 1884. 8vo.
Civil Engineers' Institution—Minutes of Proceedings, Vol. LXXVI. 8vo. 1884.
Cornwall Polytechnic Society, Royal—Fifty-first Annual Report. 8vo. 1883.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. IV. Part 3. 8vo. 1884.
De La Rue, Warren, Esq. D.C.L. F.R.S. M.R.I. et Hugo W. Müller, Ph.D. F.R.S. M.R.I. the (Authors)—Recherches Expérimentales sur la Décharge Electrique de la Pile au Chlorure d'Argent. 4^e Partie. Traduction par C. Baye. (Ann. de Chimie.) 8vo. 1884.
Dewar, Professor, M.A. F.R.S. M.R.I.—Reports on the Petroleum Trade of Baku. 8vo. 1883.
East India Association—Journal, Vol. XVI. No. 4. 8vo. 1884.
Editors—American Journal of Science for June, 1884. 8vo.
Analyst for June, 1884. 8vo.
Athenæum for June, 1884. 4to.
Chemical News for June, 1884. 4to.
Engineer for June, 1884. fol.
Horological Journal for June, 1884. 8vo.
Iron for June, 1884. 4to.
Nature for June, 1884. 4to.
Revue Scientifique and Revue Politique et Littéraire for June, 1884. 4to.
Science Monthly, Illustrated, for June, 1884.
Telegraphic Journal for June, 1884. 8vo.
Forster, Miss E. J. M.R.I.—Travels in the Interior of Southern Africa. By W. J. Burchell. 2 vols. 4to. 1822-4.
Franklin Institute—Journal, No. 702. 8vo. 1884.
Geographical Society, Royal—Proceedings, New Series, Vol. VI. No. 6. 8vo. 1884.
Geological Institute, Imperial, Vienna—Jahrbuch, Band XXXIV. No. 2. 8vo. 1884.
Grey, Henry, Esq. (the Author)—A Key to the Waverley Novels. 12mo. 1884.
Hamilton, Edward, M.D. F.L.S. M.R.I. (the Author)—Catalogue of the Engraved Works of Sir Joshua Reynolds, from 1755 to 1822. New Edition. 4to. 1884.
Some Account of the Baths of Baden in Aargau, Switzerland. 8vo. 1884.
Harris, John, Esq. (the Author)—Some Propositions in Geometry. 4to. 1884.
Johns Hopkins University—American Journal of Philology, No. 17. 8vo. 1884.
Linnean Society—Journal, Nos. 132-3. 8vo. 1884.
Lisbon, Sociedade de Geographia—Bulletin, 4^e Serie, Nos. 6, 7. 8vo. 1883.
Lord's Day Observance Society—National Conference of Friends of Lord's Day Observance. Report of Proceedings. 8vo. 1884.
Mechanical Engineers' Institution—Proceedings, No. 2. 8vo. 1884.
Meteorological Society, Royal—Communications from the International Polar Commission, Part 5. fol. 1884.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXIII. Part 4. 8vo. 1884.

Numismatic Society—Chronicle and Journal, 1884, Part 1. 8vo.
Perry, Rev. S. J. F.R.S. (the Author)—Results of Meteorological and Magnetical Observations, Stonyhurst, 1883. 12mo. 1884.
Pharmaceutical Society of Great Britain—Journal, June, 1884. 8vo.
Photographic Society—Journal, New Series, Vol. VIII. No. 8. 8vo. 1884.
Physical Society—Proceedings, Vol. VI. Part 1. 8vo. 1884.
Smith, Willoughby, Esq. M.R.I. (the Author)—Volta and Magneto-Electric Induction. (A Lecture at the Royal Institution.) 8vo. 1884.
Society of Arts—Journal, June, 1884. 8vo.
St. Pétersbourg, Académie des Sciences—Mémoires, Tome XXXI. Nos. 10–14. 4to. 1884.
Bulletins, Tome XXIX. No. 2. 4to. 1884.
Telegraph Engineers, Society of—Journal, Vol. XIII. No. 52. 8vo. 1884.
Tollemache, The Hon. Lionel A. (the Author)—Stones of Stumbling. 8vo. 1884. (Privately Printed.)
Safe Studies. 8vo. 1884. (Privately Printed.)
Tomlinson, Thomas, Esq. M.A. M.R.I. (the Author)—The Congo Treaty. 8vo. 1884.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1884: Heft 5. 4to.
Victoria Institute—Journal, No. 69. 8vo. 1884.
Zoological Society—Proceedings, 1884, Part 1. 8vo.

GENERAL MONTHLY MEETING,

Monday, November 3, 1884.

The Hon. Sir William R. Grove, M.A. D.C.L. LL.D. F.R.S.
Manager and Vice-President, in the Chair.

Charles Hartree, Esq.
Robert Wilson, Esq. C.E.

were elected Members of the Royal Institution.

William Miller Ord, M.D. was elected a Manager in the room of the late Right Hon. the Lord Claud Hamilton.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Greenwich Observations for 1882. 4to. 1884.
Greenwich Spectroscopic and Photographic Results for 1882. 4to. 1884.
The Governor-General of India—Geological Survey of India: Palæontologia Indica: Series X. Vol. III. Parts 2, 3, 4. 4to. 1884.
Records, Vol. XVII. Part 3. 8vo. 1884.

The Secretary of State for India—Synopsis of the Great Trigonometrical Survey of India, Vols. XVII.-XXI. 4to. Dehra Dun, 1883.
The British Museum—Report on the Zoological Collections of H.M.S. 'Alert,' 1881-2. 8vo. 1884.
Meteorological Office—Charts showing the Surface Temperature of the Atlantic, Indian, and Pacific Oceans. fol. 1884.
  Monthly Weather Reports for February to June, 1884. 4to.
  Quarterly Weather Reports for 1876, Parts 3 and 4. 4to. 1884.
  Hourly Readings, 1882, Part 3: and 1884, Part 2. 4to.
The French Government—Histoire Générale de Paris:
  Topographie Historique du Vieux Paris. Par F. A. Berty et L. M. Tisserand. 4to, et Plans. Paris, 1882.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Transunti. Vol. VIII. Fasc. 11-15. 4to. 1884.
American Academy of Arts and Sciences—Proceedings, Vol. XIX. Parts 1 and 2. 8vo. 1883-4.
American Philosophical Society—Proceedings, No. 115. 8vo. 1884.
Antiquaries, Society of—Archæologia, Vol. XLVIII. Part 1. 4to. 1884.
Asiatic Society, Royal—Journal, Vol. XVI. Part 3. 8vo. 1884.
Asiatic Society of Bengal—Proceedings, Nos. 3, 4, 5. 8vo. 1884.
  Journal, Vol. LIII. Part II. No. 1. 8vo. 1884.
Astronomical Society, Royal—Monthly Notices, Vol. XLIV. No. 8. 8vo. 1884.
  Memoirs, Vol. XLVIII. Part 1. 4to. 1884.
Australian Museum, Sydney—Report of the Trustees, 1883. fol. 1883-4.
Bankers, Institute of—Journal, Vol. V. Part 7. 8vo. 1884.
Batavia Observatory—Rainfall in the East Indian Archipelago, 1883. 8vo. 1884.
Bavarian Academy of Sciences—Abhandlungen, Band XV. 1te Abtheilung. 4to. 1884.
  Almanach für 1884. 12mo.
  Franz von Kobell. Von K. Haushofer. 4to. 1884.
  Gedachtnissrede auf T. L. W. von Bischoff. Von C. Kupffer. 4to. 1884.
Blaikley, D. J. Esq. (the Author)—Experiments on the Velocity of Sound in Air. (Phil. Mag. 1884.)
British Architects, Royal Institute of—Proceedings, 1883-4, Nos. 16, 17; 1884-5, No. 1. 4to.
  Transactions, 1883-4. 4to.
  List of Members, 1884. 4to.
Chemical Society—Journal for July-October, 1884. 8vo.
Civil Engineers' Institution—Minutes of Proceedings, Vol. LXXVII. 8vo. 1884.
Clinical Society—Transactions, Vol. XVII. 8vo. 1884.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. IV. Parts 4, 5. 8vo. 1884.
Dax: Société de Borda—Bulletins, 2e Serie Neuvième Année: Trimestre 2, 3. 8vo. 1884.
Duckworth, Dyce, M.D. M.R.I. (the Author)—On the Education of Medical Practitioners for Colonial Service. 8vo. 1883.
East India Association—Journal, Vol. XVI. Nos. 5, 6. 8vo. 1884.
Editors—American Journal of Science for July-October, 1884. 8vo.
  Analyst for July-October, 1884. 8vo.
  Angler's Note Book, Nos. 1, 2. 4to. 1884.
  Athenæum for July-October, 1884. 4to.
  Chemical News for July-October, 1884. 4to.
  Engineer for July-October, 1884. fol.
  Horological Journal for July-October, 1884. 8vo.
  Iron for July-October, 1884. 4to.
  Nature for July-October, 1884. 4to.
  Revue Scientifique and Revue Politique et Littéraire for July-October, 1884. 4to.
  Science Monthly, Illustrated, for July-October, 1884.
  Telegraphic Journal for July-October, 1884. 8vo.

Franklin Institute—Journal, Nos. 703–706. 8vo. 1884.

Galloway, Robert, Esq. F.C.S. (the Author)—1. Extraction of Sugar from the Juice of the Cane and Beet Root: 2. Emerald Green. (Journal of Science, 1884.)

Geographical Society, Royal—Proceedings, New Series, Vol. VI. Nos. 7–10. 8vo. 1884.

Geological Institute, Imperial, Vienna—Jahrbuch, Band XXXIV. Heft 3. 8vo. 1884.

Geological Society—Quarterly Journal, No. 159. 8vo. 1884.

Gill, David, Esq. LL.D. F.R.S. and W. L. Elkin, Esq. Ph.D. (the Authors)—Heliometer-Determinations of Stellar Parallax in the Southern Hemisphere. (Mem. of Astron. Soc. Vol. XLVIII.) 1884.

Glasgow Philosophical Society—Proceedings, Vol. XV. 8vo. 1884.

Grey, Henry, Esq. (the Author)—Trowel, Chisel, and Brush; a concise Manual of Architecture, Sculpture, and Painting. 12mo. 1884.

Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Tome XIX. Liv. 2. 8vo. 1884.

International Health Exhibition—Catalogue of the Library. 8vo. 1884.

Jablonowski'sche Gesellschaft, Leipzig, Furstliche—Preisschrift, No. 24. 4to. 1884.

Johns Hopkins University—American Chemical Journal, Vol. VI. No. 3. 8vo. 1884.
American Journal of Philology, No. 18. 8vo. 1884.
University Circulars, Nos. 31, 32. 4to. 1884.

Linnean Society—Journal, Nos. 103–5, 134. 8vo. 1884.
Transactions: Zoology, 2nd Ser. Vol. II. Part 10; Botany, 2nd Ser. Vol. II. Part 7. 4to. 1884.

Leeds Philosophical and Literary Society—Report, 1883–4. 8vo.

Lisbon, Sociedade de Geographia—Bulletin, 4e Serie, Nos. 8, 9. 8vo. 1883.
Expedição Scientifica á Serra da Estrella em. 1881. 4to. 1883.

Liverpool Polytechnic Society—Annual Report and Journal, 1883. 8vo.

Lloyd, Wm. Watkiss, Esq. M.R.I. (the Author)—Much Ado About Nothing. By W. Shakespeare. With a Prefatory Essay. 8vo. 1884.

McCutcheon, W. G. Esq. (the Editor)—The Atlantic Ocean, Vol. I. No. 1. 8vo. 1884.

Madrid Scientific and Literary Athenæum—Catálogo de las Obras Existentes en la Biblioteca. 8vo. 1873.
Discurso por Marqués de Molins. 8vo. 1874.
Centenario de Calderon. Disertaciones, Poesias y Discursos. 8vo. 1881.
Velada en D. José Moreno Nieto. 8vo. 1882.
Discursos Academicos de Moreno Nieto. Precedidos de un Discurso de D. A. Cánovas del Castillo. 8vo. 1882.
Obras de D. Manuel de la Revilla. 8vo. 1883.
Discursos leidos en el Ateneo. 8vo. 1884.
Curso de Ciencias Naturales Conferencias, 1882. 8vo. 1883.
Curso de Historia Universal Conferencias, 1882. 8vo. 1883.

Magrath, Miss E.—The Chemical Catechism. By S. Parkes. 3rd edition. 8vo. 1808.
The Edinburgh New Dispensatory. By A. Duncan. 5th edition. 8vo. 1810.
[These Books were bound by Prof. Faraday for Mr. J. Huxtable, Chemist.]

Manchester Geological Society—Transactions, Vol. XVII. Parts 16–18. 8vo. 1883–4.

Manchester Steam Users' Association—Reports, 1883. 8vo.
Boiler Explosions Act, 1882. Board of Trade Reports, &c. fol. 1884.

Maryland Medical and Chirurgical Faculty—Transactions, 86th Session. 8vo. 1884.

Medical and Chirurgical Society—Proceedings, New Series, No. 6. 8vo. 1884.

Meteorological Society, Royal—Quarterly Journal, No. 50. 8vo. 1884.
Meteorological Record, No. 12. 8vo. 1883.
Communications from the International Polar Commission, Part 6. fol. 1884.

Metropolitan Association for Improving the Dwellings of the Industrial Classes—Reports, &c. fol. and 8vo. 1841–1884.

Middlesex Hospital—Reports for 1882. 8vo. 1884.
National Association for Social Science—Proceedings, Vol. XVII. No. 3. 8vo. 1884.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXIII. Part 5. 8vo. 1884.
Norwegian North-Atlantic Expedition, Editorial Committee—Danielssen, D. C. and Koren, J. Asteroidea. fol. Christiania, 1884.
Numismatic Society—Chronicle and Journal, 1884, Part 2. 8vo.
Pharmaceutical Society of Great Britain—Journal, July-October, 1884. 8vo.
Photographic Society—Journal, New Series, Vol. VIII. No. 9; Vol. IX. No. 1. 8vo. 1884.
Physical Society—Proceedings, Vol. VI. Part 2. 8vo. 1884.
Pollock, Sir Frederick, Bart. M.A. M.R.I.—Tenth Census of the United States (June 1, 1880): Vol. I. Population; Vol. II. Manufactures; Vol. III. Agriculture. 4to. 1883.
Preussische Akademie der Wissenschaften—Sitzungsberichte, XVIII.-XXXIX. 4to. 1884.
Radcliffe Observatory—Radcliffe Observations for 1881. 8vo. 1884.
Richardson, B. W. M.D. F.R.S. (the Author)—The Asclepiad. Vol. I. Nos. 3, 4. 8vo. 1884.
Rio de Janeiro, Observatoire Imperiale—Bulletin, No. 12. fol. 1883.
Royal College of Surgeons of England—Calendar, 1884. 8vo.
Royal Society of Canada—Proceedings and Transactions, Vol. I. (1882-3). 4to. Montreal, 1883.
Royal Dublin Society—Transactions, Vol. I. Nos. 20 to 25; Vol. III. Nos. 1 to 3. 4to. 1882-4.
Proceedings, Vol. III. Parts 6 and 7; Vol. IV. Parts 1 to 4. 1882-4.
Royal Society of Edinburgh—Transactions, Vol. XXX. Parts 2 and 3. 4to. 1881-3.
Proceedings, 1881-3. 8vo.
Royal Society of London—Proceedings, Nos. 231, 232. 8vo. 1884.
Russell, The Hon. F. A. M.A. F.R.M.S. M.R.I. (the Author)—The Sunsets and Sunrises of Nov. 1883 to Jan. 1884. (Journal of Meteorological Society, 1884.)
St. Pétersbourg, Académie des Sciences—Mémoires, Tome XXXI. Nos. 15, 16; Tome XXXII. Nos. 1-3. 4to. 1884.
Sanitary Assurance Association—Sanitary Arrangements of Dwelling Houses. By Mark H. Judge. 8vo. 1884.
Seismological Society of Japan—Transactions. Vol. VII. Part 1. 8vo. 1884.
Smithsonian Institution, Washington—Annual Report for 1882. 8vo. 1884.
Society of Arts—Journal, July-October, 1884. 8vo.
Society for Psychical Research—Proceedings, Vol. I. Part 6. 8vo. 1884.
Statistical Society—Journal, Vol. XLVII. Parts 2, 3. 8vo. 1884.
Catalogue of the Library. 4to. 1884.
Tasmania Royal Society—Reports for 1882 and 1883. 8vo. 1883-4.
Tegima, S. Esq. (Japanese Commissioner)—General Outlines of Education in Japan. 8vo. 1884.
Telegraph Engineers, Society of—Journal, Vol. XIII. No. 53. 8vo. 1884.
Tokio University—Memoirs, No. 10. 8vo. 1884.
United Service Institution, Royal—Journal, Nos. 124, 125. 8vo. 1884.
Upsal University—Nova Acta, Ser. III. Vol. XII. Fasc. 1. 4to. 1884.
Bulletin Mensuel de l'Observatoire Météorologique, Vol. XV. 4to. 1883-4.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1884: Heft 6, 7. 4to.
Victoria Institute—Journal, No. 70. 8vo. 1884.
White, H. William, Esq. (the Author)—Architecture and Public Buildings. 8vo. 1884.
Yorkshire Archæological and Topographical Association—Journal, Part 32. 8vo. 1884.
Zoological Society—Proceedings, 1884, Parts 2, 3. 8vo.
List of the Fellows. 8vo. 1884.

GENERAL MONTHLY MEETING,

Monday, December 1, 1884.

The Hon. Sir William R. Grove, M.A. D.C.L. LL.D. F.R.S.
Manager and Vice-President, in the Chair.

Sir Frederick A. Abel, K.C.B. D.C.L. F.R.S.
George Andrews, Esq. J.P.
George Mann Carfrae, M.D.
Rogers Field, Esq.
James P. Harper, M.D.
Samuel Page, Esq.
Boverton Redwood, Esq. F.C.S. F.I.C.
Thomas Alfred Routh, Esq.
J. Graham Smith, Esq.

were elected Members of the Royal Institution.

A Report from the Managers was read stating that it had been found necessary to reconstruct the whole of the drainage system of the Institution, which had been effected upon the most approved principles under the advice and supervision of Mr. Rogers Field, the eminent sanitary engineer.

The following Lecture Arrangements were announced:—

Professor Tyndall, D.C.L. F.R.S. *M.R.I.*—Six Lectures (adapted to a Juvenile Auditory) on The Sources of Electricity: Friction-electricity, Volta-electricity, Pyro-electricity, Thermo-electricity, Magneto-electricity; on Dec. 27 (Saturday), Dec. 30, 1884; Jan. 1, 3, 6, 8, 1885.

Professor Henry N. Moseley, M.A. F.R.S.—Five Lectures on Colonial Animals: their Structure and Life Histories; on Tuesdays, Jan. 13, 20, 27, Feb. 3, 10.

Professor Sidney Colvin, M.A.—Two Lectures on Museums and National Education; on Tuesdays, Feb. 17, 24.

Professor Arthur Gamgee, M.D. F.R.S.—Four Lectures on Digestion (the Subject to be continued after Easter); on Tuesdays, March 3, 10, 17, 24.

Professor Dewar, M.A. F.R.S. *M.R.I.*—Eleven Lectures on The New Chemistry; on Thursdays, Jan. 15, 22, 29, Feb. 5, 12, 19, 26, March 5, 12, 19, 26.

Charles Waldstein, Ph.D. Heidelberg, Hon. M.A. Cantab.—Three Lectures on Greek Sculpture from Pheidias to the Roman Era; on Saturdays, Jan. 17, 24, 31.

George Johnstone Stoney, Esq. F.R.S.—Three Lectures on The Scale on which Nature works, and the Character of some of her Operations; on Saturdays, Feb. 7, 14, 21.

Carl Armbruster, Esq.—Five Lectures on The Life, Theory, and Work of Richard Wagner (with Illustrations, Vocal and Instrumental); on Saturdays, Feb. 28, March 7, 14, 21, 28.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The French Government—Dictionnaire Topographique du Departement des Hautes Alpes. Par J. Roman. 4to. 1884.

Inventaire des MSS. de la Bibliothèque Nationale. Fonds de Cluni. Par L. Delisle. 8vo. 1884.

The Lords of the Admiralty—Nautical Almanac for 1888. 8vo. 1884.

Agricultural Society of England, Royal—Journal, Vol. XX. Part 2. 8vo. 1884.

Asiatic Society of Bengal—Proceedings, Nos. 6, 7. 8vo. 1884.

Journal, Vol. LIII. Part II. No. 2. 8vo. 1884.

Astronomical Society, Royal—Monthly Notices, Vol. XLIV. No. 9. 8vo. 1884.

Bankers, Institute of—Journal, Vol. V. Part 8. 8vo. 1884.

Bell, I. Lowthian, Esq. F.R.S. (the Author)—Manufacture of Iron and Steel. 8vo. 1884.

British Architects, Royal Institute of—Proceedings, 1884–5, Nos. 2, 3. 4to.

Canada Meteorological Office—Report, 1882. 8vo.

Chemical Society—Journal for November, 1884. 8vo.

Civil Engineers' Institution—Minutes of Proceedings, Vol. LXXVIII. 8vo. 1884.

Devonshire Association for the Advancement of Science, Literature, and Art—Report and Transactions, Vol. XVI. 8vo. 1884.

The Devonshire Domesday, Part 1. 8vo. 1884.

Editors—American Journal of Science for November, 1884. 8vo.

Analyst for November, 1884. 8vo.

Athenæum for November, 1884. 4to.

Chemical News for November, 1884. 4to.

Engineer for November, 1884. fol.

Horological Journal for November, 1884. 8vo.

Iron for November, 1884. 4to.

Nature for November, 1884. 4to.

Revue Scientifique and Revue Politique et Littéraire for November, 1884. 4to.

Science Monthly, Illustrated, for November, 1884.

Telegraphic Journal for November, 1884. 8vo.

Franklin Institute—Journal, No. 707. 8vo. 1884.

Geographical Society, Royal—Proceedings, New Series, Vol. VI. No. 11. 8vo. 1884.

Geological Society—Quarterly Journal, No. 160. 8vo. 1884.

Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Tome XIX. Liv. 3. 8vo. 1884.

Iron and Steel Institute—Journal for 1884, No. 1. 8vo.

Madrid Scientific and Literary Athenæum—Discurso leido por D. S. M. Y. Prendergast. 8vo. 1884.

Manchester Geological Society—Transactions, Vol. XVIII. Parts 1 and 2. 8vo. 1884.

Mechanical Engineers' Institution—Proceedings, No. 3. 8vo. 1884.

Medical and Chirurgical Society, Royal—Transactions, Vol. LXVII. 8vo. 1884.

Meteorological Office—Monthly Weather Report, August 1884. 4to.

Meteorological Society, Royal—Quarterly Journal, No. 51. 8vo. 1884.

Meteorological Record, No. 13. 8vo. 1884.

Norfolk and Norwich Naturalists' Society—Transactions, Vol. III. Parts 4 and 5. 8vo. 1883–4.

Odontological Society of Great Britain—Transactions, Vol. XVII. No. 1. New Series. 8vo. 1884.

Pharmaceutical Society of Great Britain—Journal, November, 1884. 8vo.

Royal Society of London—Proceedings, No. 233. 8vo. 1884.

Royal Society of New South Wales—Journal and Proceedings, Vol. XVII. 8vo. 1884.

Society of Arts—Journal, November, 1884. 8vo.

St. Pétersbourg, Académie des Sciences—Bulletins, Tome XXIX. No. 3. 4to. 1884.

Teyler Museum—Archives, Série II. Vol. 2. Part 1. 4to. 1884.

United States Geological Survey—Mineral Resources of the United States. By A. Williams. 8vo. 1883.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1884: Heft 8. 4to.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 16th, 1885.

The Duke of Northumberland, D.C.L. LL.D. President, in the Chair.

PROFESSOR TYNDALL, D.C.L. LL.D. F.R.S. *M.R.I.*

On Living Contagia.

By desire of our excellent Honorary Secretary, Sir William Bowman, to all of whose requests I wish to pay dutiful attention, I have incurred the risk of standing before you here to-night. I speak thus, because the time at my disposal, since the conclusion of the Christmas lectures, has been far from adequate to the preparation of this discourse. Its subject will, in the main, be a review of the labours of Pasteur. Before he was born, Arago and Biot had discovered that rock crystal, cut in a certain direction, possessed the extraordinary power of rotating the plane of polarized light. Some samples of the crystal turned the plane of polarization to the right, and others to the left; they were therefore called respectively, right-handed and left-handed crystals. The power disappeared when the crystal was dissolved, and did not therefore reside in the molecule or unit-brick of the crystal. Biot afterwards discovered that many liquids possessed this power of rotation; and in these cases the rotary force must have resided in the molecule. Two compounds of tartaric acid had been discovered which, in solution, turned the plane of polarization, the one to the right, and the other to the left. The left-handed tartrate was discovered by Pasteur. Prompted by an observation made in Germany, Pasteur mixed the pure, right-handed, tartrate of ammonia with some albumenoid substances, and exposing the mixture to a gentle heat, found that it fermented. The solution, at first limpid, became turbid, and this turbidity was proved to be due to the multiplication in the liquid of a minute microscopic fungus. A solution was then prepared, wherein the right-handed and left-handed tartrates were so equally balanced that they completely neutralized each other. The solution had at first no power over polarized light. But immediately after fermentation had begun, or in other words, after the fungus had begun to multiply, a rotation to the left was observed. This increased until it reached a maximum, when it was found that all the right-handed tartrate had disappeared from the liquid, leaving the left-handed tartrate behind. Now the two tartrates were alike in chemical composition; they possessed the same atoms and the same proportion of atoms; but owing to a difference in the structure of their molecules, one of them turned the plane of polarization to the right and the other to the left. This self-same molecular peculiarity enabled the little fungus to live and thrive upon the one class of molecules, while leaving the other class intact.

In making these extremely curious observations, Pasteur found himself confronted by the general question of fermentation. In 1837, Cagniard-Latour and Schwann had independently proved the alcoholic ferment to be a budding microscopic vegetable. Spurred by this discovery and strengthened by his own, Pasteur rapidly closed with the idea that all ferments were living organisms, and that the substances usually regarded as ferments were in reality the food of the ferments. Thus the sugar of the wort in the manufacture of beer, the sugar of the grape in the manufacture of wine, the sugar of the cherry in the manufacture of kirsch, the sugar of the apple in the manufacture of cider, and, it might perhaps be added, the sugar of the gooseberry in the manufacture of champagne, is decomposed by the little organism which derives from the sugar the oxygen necessary for its existence. One of the products of this decomposition is our familiar alcohol.

When I studied at the University of Marburg, one of the luxuries of student life consisted of pancakes and sour milk. Whence this pleasant acidity? It was proved by Pasteur to be due to living microscopic rods—the lactic acid ferment—which grew and multiplied in the milk. The butyric acid ferment was also proved to be an organism. The acidity of sour wines was proved to be due to a minute microscopic plant called *Mycoderma aceti.* By the action of this plant, wine is transformed into vinegar, the vast industries of Orleans and other places being based upon its operations. Ruinous losses had frequently been incurred by the souring of French wines, but Pasteur proved to the wine-grower that by simply heating his wine to a temperature of 122° Fahrenheit—a temperature which in no way alters the quality of the wine—the injurious organisms are all destroyed, the wine being thereby permanently protected.

The sourness, putridity, and other maladies of beer were traced by Pasteur to special organisms—ferments of disease, as he rightly calls them—which, mingling with the torula or true yeast plant, added their offensive products to the pure alcohol. Vast losses were frequently incurred by the use of bad yeast, where five minutes' examination with the microscope would have revealed to the brewer the cause of the badness, and prevented him from using the yeast. Under the head of fermentation, Pasteur rightly placed the phenomena of putrefaction, which he studied with admirable thoroughness and skill. All the objectionable odours of putrefying flesh result from decompositions set up by microscopic organisms. Keep your meat free from such organisms—kill them by heat or deaden them by cold—and you can have no putrefaction. Schwann, whom I have already mentioned, was the first to upset the doctrine propounded by Gay Lussac, that putrefaction was caused by atmospheric oxygen, and to prove that it was not the air, but living germs suspended in the air which caused flesh to putrefy. To his succeeded the far more elaborate labours of Pasteur. Note the unlooked-for issues to which these labours have led. With the boldness and penetration which belong to genius, Lister extended to living matter the generalisation established by Schwann and Pasteur in regard to dead matter. With admirable clearness of vision, he pictured the atmospheric germs falling upon the wounds in our hospitals and setting at nought, by subsequent mortification, the skill of the best operator. Lister insisted that the treatment after the operation was quite as important as the operation itself. He devised effectual means for destroying these putrefactive organisms, and thus established that antiseptic system of surgery which is one of the greatest and most beneficent achievements of the age in which we live.

We now stand upon the margin of a new field which invited the activity of Pasteur. In 1865, owing to a plague among the worms, the silk husbandry of France had fallen into ruin. The worms sickened and died wholesale; and, because of the spots upon their skin, the malady was called Pébrine. A minute corpuscular organism, called *Micrococcus ovatus*, had been discovered by Cornalia in the blood and organs of the diseased worms, and, acquainted as he was with the action of living ferments, Pasteur was prepared to see in the corpuscles the cause of the plague. He followed them through all the phases of the insect's life—through the egg, through the worm, through the chrysalis, through the moth. He proved that the germ of the malady might be present in the egg, and still escape observation. In the worm also it might elude the search of the microscope. But in the moth it reached a development so distinct as to render its detection inevitable. From healthy moths, healthy eggs were sure to spring; from healthy eggs vigorous worms, from vigorous worms fine cocoons; so that the great problem of restoring to France its silk industry was reduced by Pasteur to the separation of the healthy from the unhealthy moths, the destruction of the latter and the exclusive employment for breeding of the eggs of the former.

While discovering the cause of *pébrine* and the way to combat it, Pasteur inquired how the disease was spread; pursuing in this inquiry the only way open to the investigator. He infected healthy worms by smearing with corpusculous matter the mulberry leaves on which they fed. He infected them by inoculation, and showed how they infected each other by the wounds and scratches of their own claws. Bringing together healthy and diseased worms, the healthy ones, like their smitten companions, soon sickened and died. He produced infection at a distance, by wafting corpusculous dust through the air. All the modes by which infection is spread among human beings were thus illustrated. Was it cruel to treat the healthy silkworms in this fashion? It may have been for the moment; but Pasteur's investigation swept a deadly epidemic from the soil of France; and, for the units slain during the inquiry, millions have been preserved.

In May 1876, there appeared in Cohn's 'Beiträge zur Pflanzen-

Zoologie' a memoir on a disorder called by the French *charbon*, by the Germans *Milzbrand*, and by the English, woolsorters' disease, malignant pustule, or splenic fever. This memoir seemed to me to mark an epoch in the history of a most deadly malady, and also in the history of the germ theory itself. The contagium of splenic fever is an organism which appears as rods in the blood and tissues of men and animals smitten by the fever. The name of the organism is *Bacillus anthracis*, and with consummate skill, patience, and penetration, in the memoir referred to, its life-history was completely followed out. It was easy to predict that the author of the paper, who at that time held a small appointment in the neighbourhood of Breslau, would soon find himself transferred to a higher position. The admirable worker to whom I here refer was Dr. Koch, and the next time I heard of him he was head of the Imperial Sanitary Institute of Berlin.

Koch was not the discoverer of the parasite of splenic fever. As early as 1850, Davaine and Reyer had observed the microscopic rods in the blood of animals that had died of the disease. But they were at the time unconscious of the significance of their observation, and for thirteen years allowed the matter to drop. Roused by the researches of Pasteur, Davaine returned to the subject in 1863, and then affirmed the rods to be the contagium of the fever. He was opposed by some of his own countrymen, and hot discussions on the subject were carried on in the Academy of Sciences and elsewhere. Pollender, Brauell, and Burdon Sanderson made important contributions to the etiology of the disease; but knowledge was contradictory, uncertain, and incomplete till Koch grasped the subject in 1876, and, with the ardour of a youth and the caution of a veteran in microscopic research, cultivated the organism, reconciled contradictions, and placed beyond the possibility of doubt the parasitic character of splenic fever. This result again was achieved by the only means open to the investigator; namely, by the inoculation of healthy animals with a living virus derived either from artificial cultivations or from other animals smitten with the disease. An interval of twenty-six years from the first observation of Davaine, was closed by Koch's memorable investigation.

Pasteur long held himself aloof from any direct examination of the germ theory. But he must have been profoundly impressed by his own experiments on silkworms, and he was at length made aware of the vast promise of the field of inquiry which his own researches and those of others had opened up. Attacking the subject of splenic fever, he soon gave evidence of the genius which characterised his former work. Take an illustration. Koch had proved that while mice and guinea pigs were infallibly killed by *Bacillus anthracis*, birds were able to defy it. Why? Here is the answer given by Pasteur. The higher limit of temperature which arrests the multiplication of the bacillus in infusions is 44° Cent. (111° Fahr.) The temperature of the blood of birds is from 41° to 42° Cent. It is therefore close to the prohibitary temperature. Now the blood-corpuscles of a living fowl are sure to offer a certain resistance to any attempt to deprive them of their oxygen. But close to its prohibitory temperature, the anthrax contagium may be expected to be enfeebled, and the question arises: May not the blood-corpuscles in these circumstances be able successfully to combat the organism? Experiment alone could decide, and Pasteur made the experiment. He lowered, by chilling in cold water, the temperature of a fowl 4°; and while in this condition inoculated it with the splenic fever parasite. In twenty-four hours the fowl was dead. Inoculating another fowl similarly chilled, he permitted the fever to come to a head, and then transferred the fowl to a warm chamber. The career of the parasite was soon stopped, and in a few hours the health of the fowl was restored. The issues of this experiment, as suggesting the application of heat or cold in the fevers which afflict humanity, are incalculable.

Pasteur next attacked the fatal malady of chicken cholera. The parasite of this disease had been discovered before him; but, by a method now universally applied, he rendered the solution of the problem sure. Into chicken broth, boiled sufficiently long to destroy all germs with which it may be contaminated, let a minute drop of the blood of a fowl suffering from chicken cholera be introduced. The organisms of the blood increase and multiply, until they finally invade the whole of the nutritive liquid. With the point of a needle, let a speck of this liquid be introduced into a second sterilised infusion. As before, the organisms multiply until the liquid becomes thick with them. Let a speck, taken on the needle-point from this second cultivation, be communicated to a third sterilised infusion. A similar result will be observed. In this way any number of generations of the organism may be cultivated, and whatever foreign matter, chemical poison, or otherwise, might attach itself to the drop of blood first taken from the animal, is thus washed away. This is the method of "pure culture" now pursued. After fifty cultures, let a speck of the infected infusion be introduced into the blood of a healthy fowl. Cholera and death are the consequence. The organism is as virulent as at first. But here we come to a point of supreme importance. The virulence is maintained on one condition. The cultures must rapidly succeed each other. When the infusion with its swarming organisms is allowed to remain for a week, for a fortnight, for a month, for two months, without renewal, the power of the organism gradually diminishes, and after a certain time, though it may be able to produce *malaise*, it is not able to produce death. It thus becomes what Pasteur calls an attenuated virus—a true vaccine. For, if with the organism in this condition, a fowl be inoculated, the subsequent malaise passes away, and the fowl is afterwards proof against the organism in its most virulent form.

Pasteur next laid hold of the murderous virus of splenic fever, and succeeded in rendering it not only harmless to life, but a sure protection against the assaults of the disease. It was soon noised abroad among the sheep and cattle dealers of France that he had overcome this contagium. In various districts of France the disorder was very deadly. He confined himself for a time, to what might be called laboratory experiments; but believing that a principle which had proved true in small things would also prove true in large, he had the boldness to accept an invitation from the President of the Agricultural Society of Melun, to make an experiment publicly, on what might be called an agricultural scale.

He had placed at his disposal a flock of sheep which he divided into two groups. The members of one group were all vaccinated with the "attenuated" virus of splenic fever, while the members of the other group were left unvaccinated. A number of cows were similarly treated. The question to be decided was: Would the mild virus act as a protection? Experiment alone could answer this question. Fourteen days subsequent to the first inoculation, all the sheep and all the cows, vaccinated and unvaccinated, were inoculated with a highly virulent virus. Three days afterwards, more than two hundred persons, including among them journalists, farmers, lawyers, and public men, assembled to witness the result. Pasteur is capable of elation, and he must have felt elated at the shout of admiration which hailed the success of his experiment. Of 25 sheep inoculated with a virulent virus, but unprotected by vaccination, 21 were already dead, while the remaining 4 were dying. The 25 vaccinated sheep, which had also received the deadly virus into their blood, were in full health and gaiety. The unvaccinated cows showed tumours at the place of inoculation, and were so prostrate with fever as to be unable to eat. The vaccinated cows remained perfectly well, showing neither tumours nor fever, nor even any rise of temperature, and consumed their food with appetites unimpaired. Pasteur was soon overwhelmed with applications for this "benign vaccine." At the end of 1881, close upon 34,000 animals had been vaccinated, while in 1883 the number rose to nearly 500,000.

Malignant pustule is a very loathsome disease, and the sufferings of animals dying from it are obviously very great. An account of the symptoms which precede death would be by no means pleasant reading. Imagine then one of those tender-hearted gentlemen who write about torture and cruelty in the *Times*, entering the laboratory of Pasteur and seeing him sow this malady among his unprotected victims! Some years ago, accompanied by my wife, I visited the laboratory of the École Normale, and we were shown there by Pasteur himself, the formidable organism in the investigation of which he was then engaged. It was curious to reflect how a thing so mean could exercise such deadly power over man and brute. Both Pasteur and his assistants had to be very wary in dealing with this organism, for either the adult bacillus, or one of its spores, entering the blood by the slightest scratch on the skin, would have proved fatal to the individual infected by it. In a room adjacent to the laboratory stood a large cage, containing guinea-pigs and rabbits, some sprightly, and munching their food; some drowsy and languid; some mortally sick, some in the last agony, and some in the rigor of death. It was subsequent to these experiments that Pasteur operated on larger animals, subjecting them to the "tortures" I have just described. What would a tender-hearted bishop * have said under these circumstances? Were it in his power to do so, would he not have invoked the arm of the law to stay such damning cruelty? Most assuredly he would. And yet, in doing so, he would have affixed the brand of cruelty upon himself. In lieu of the few animals saved from the operations of the man of science, he would have handed over tens of thousands of the same animals to the fearful ravages of splenic fever.

[J. T.]

---

* The Bishop of Oxford had been just writing to the *Times* on the cruelties of experimental physiology.

WEEKLY EVENING MEETING,

Friday, January 23, 1885.

Sir Frederick Bramwell, F.R.S. Manager and Vice-President, in the Chair.

Professor H. N. Moseley, M.A. F.R.S.

The Fauna of the Seashore.

The marine fauna of the globe may be divided into the littoral, the deep-sea, and the pelagic faunas. Of the three regions inhabited by these faunas, the littoral is the one in which the conditions are most favourable for the development of new forms through the working of the principle of natural selection. As Professor Lovèn writes, "The littoral region comprises the favoured zones of the sea where light and shade, a genial temperature, currents changeable in power and direction, a rich vegetation spread over extensive areas, abundance of food, of prey to allure, of enemies to withstand or evade, represent an infinitude of agents competent to call into play the tendencies to vary which are embodied in each species, and always ready by modifying its parts to respond to the influences of external conditions." It is consequently in this littoral zone, where the water is more than elsewhere favourable for respiration, and where constant variation of conditions is produced by the tides, that all the main groups of the animal kingdom first came into existence; and here also, probably, where the first attached and branching plants were developed, thus establishing a supply of food for the colonisation of the region by animals.

The animals inhabiting the littoral zone are most variously modified, to enable them to withstand the peculiar physical conditions which they encounter there. Hence the origin of all hard shells and skeletons of marine invertebrata, various adaptations for boring in sand, the adoption of the stationary fixed condition, and similar arrangements. Almost all the shore forms of animals, however inert in the adult condition, pass through in embryological development free-swimming larval stages which are closely alike in form for very widely different groups of animals. Thus the oyster and most other mollusca of all varieties and shapes when adult, develop from a free-swimming pelagic trochosphere larva, and so do many annelids. Such larvæ cannot be of subsequent origin to the adults of which they are phases. If such were the case, they would not have become so closely alike in structure. In reality they represent the common ancestors from which all the forms in which they occur were derived, and as all these larvæ are pelagic in habits and structure, it follows that the inhabitants of the shores were derived from pelagic ancestors. The earliest plants were also probably free-swimming.

In the case of the cirripedia there can be no doubt, from the history of their development, that they were originally pelagic, and have become specially modified for coast life; and in the case of the echinoderms the only possible explanation of the remarkable similarity of the larval forms of the various groups of widely differing adults is that these pelagic larvæ represent a common ancestor of the group. The madreporarian corals all spring from a pelagic larva. The colonial forms probably owe their origin and that of their skeletons to the advantage gained by them in the formation of reefs, and the increase in facilities of respiration consequent on the production of surf. In the deep sea they are very scarce.

The vertebrata are sprung from a very simple free-swimming ancestor, as shown by the ciliated gastrula stage of Amphioxus. The ascidians afford another evident instance of the extreme modification of pelagic forms for littoral existence.

The peculiar mode of respiration of vertebrata by means of gill-slits occurs in no other animal group except in Balanoglossus, which will probably shortly be included amongst vertebrata. Possibly gill-slits as a respiratory apparatus first arose in a littoral form, such as Balanoglossus, and hence their presence at the anterior end of the body, that nearest to the surface in an animal buried in sand. The connection of Balanoglossus with the echinoderms through Tornaria is very remarkable. Possibly Amphioxus once had a Tornaria stage, and has lost it just as one species of Balanoglossus has lost it, as Mr. Bateson has lately discovered.

The littoral zone has given off colonists to the other three faunal regions. The entire terrestrial fauna has sprung from colonists contributed by the littoral zone. Every terrestrial vertebrate bears in its early stages the gill-slits of its aquatic ancestor. All organs of aerial respiration are mere modifications of apparatus previously connected with aquatic respiration, excepting, perhaps, in the case of Tracheata, tracheæ being most likely modifications of skin-glands, as appears probable from their condition in Peripatus. The oldest known air-breathing animals are insects and scorpions, which have lately been found in Silurian strata. Prof. Ray Lankester believes the lungs of scorpions to be homogenous with the gill-plates of Limulus. Birds were possibly originally developed in connection with the seashore, and were fish-eaters like the tooth-bearing Hesperornis.

The fauna of the coast has not only given rise to the terrestrial and fresh-water fauna; it has from time to time given additions to the pelagic fauna in return for having thence derived its own starting-points. It has also received some of these pelagic forms back again, to assume afresh littoral existence.

The deep-sea fauna has probably been formed almost entirely from the littoral, not in the remotest antiquity, but only after food derived from the débris of the littoral and terrestrial faunas and floras became abundant.

It is because all terrestrial and deep-sea animal forms have passed through a littoral phase of existence, and that the littoral animals retain far better than those of any other faunal region the recapitulative larval phases by means of which alone the true histories of their origins can be recovered, that marine zoological laboratories on the coast have made so many brilliant discoveries in zoology during late years.

The lecturer concluded by appealing for assistance, in the way of subscriptions to the funds of the Marine Biological Association of Great Britain, the object of which is to construct a marine laboratory on the English coast for the purpose of researches such as those referred to. England is at present without any such laboratory, although nearly all Continental countries possess them.

[H. N. M.]

WEEKLY EVENING MEETING,

Friday, January 30, 1885.

Sir Frederick Pollock, Bart. M.A. Manager and Vice-President,
in the Chair.

Professor Ernst Pauer.

A short review of the Works of Living Composers for the Pianoforte.

The Musical Illustrations were given by Mr. Max Pauer.

The lecturer began by observing that it is a matter of great difficulty to become acquainted with new musical works; with musicians it is not like with authors, whose works are reported and criticised in the different magazines, and are afterwards perused by an eager public through the means of the circulating libraries. It requires a great amount of technical efficiency and musical knowledge to execute and read satisfactorily a new pianoforte piece—and even when such physical and mental powers are possessed, it is generally only a very small circle which can profit by it.

In the public concerts a very strong feeling of conservatism exists with regard to the composition of concert-programmes, the general public really and truly not caring to listen to new works of composers whose names are not yet universally accredited. In this respect, the painters and sculptors enjoy a most decided advantage—their works are to be seen either in the great annual Exhibition of the Royal Academy or in one or the other of the numerous smaller galleries, and thus the public is able to become acquainted with the talent and genius of English, French, Belgian, German, Italian, Russian, Norwegian, &c., painters; excellent notices are written by competent critics and appear in the principal daily papers, enabling the public who are fond to be guided in such matters, to detect what is to be admired and praised, and what on the other hand may be treated with comparative indifference. To hold a similar small exhibition of musical cabinet-pictures and to add some verbal critical notes is attempted this evening. The chosen works show the individuality and characteristic talent of the composers. Several well-known and justly famous composers — still living — have been omitted — for instance Franz Liszt, Wilhelm Taubert, Stephen Heller, Adolph Henselt, Niels Gade—all these musicians have attained an age ranging between 65 and 75 years, and thus belong more to the past than to the present generation. It is also necessary to leave out several highly esteemed younger composers, such as Friedrich Gernsheim, Woldemar Bargiel, Salomon Jadassohn, Julius Schulhoff, Camille St. Saëns, Heinrich Hofmann—but their turn may come on a future occasion.

The spirit and expression of our present pianoforte-music is decidedly that of elegance, more cleverness than feeling, and a very carefully considered refinement. None of the present composers possesses the genius of a Mozart, Beethoven or Schubert; but they may lay claim to a talent of the highest order, and have by earnest study, undaunted perseverance, and accomplishment certainly attained a high degree of artistic excellence. All the present composers have more or less been influenced by Mendelssohn, Chopin, Schumann, and Richard Wagner. Some of the works are made up from apparently very insignificant material, but the cleverness with which this material is handled is so conspicuous that to the less experienced ear it sounds like the product of a great genius. We begin our review with the celebrated

1. ANTON RUBINSTEIN

(Born November 30, 1830, at Wechwotynez, near Jassy).

(a) *Nocturne* in G.
(b) *Capriccio* in E flat.

The *Nocturne* is full of sweet expression, slightly tinged with exquisite melancholy; it is more remarkable for the euphony of its tone effect than for the richness and solidity of its content. The *Capriccio* is a crisp, interesting and sparkling Scherzo, which exhibits greater care in thematic work than Rubinstein generally devotes to his compositions. The characteristic qualities of the celebrated composer are a strong and impulsive feeling, a certain amount of elegance, and undoubted energy.

NO. 2.—JOHANNES BRAHMS

(Born May 7, 1833, at Hamburg).

(a) *Intermezzo* in A♭ flat.
(b) *Capriccio* in B Minor.

The same high position Richard Wagner holds in the domain of *dramatic music*, Brahms occupies in the realm of *instrumental* music; the basis on which Brahms's works rest is a scientific one, but it is a science suffused with modern feeling and the harmonies which complete the scientific matter are such as derive their origin from the *romantic* phase of our art. There is an intellectual agency in his pieces which will perhaps not strike the listener at once, but which is found out by degrees. The *Intermezzo* is actually a mere sketch, which however, is complete in itself, full of charm, and showing a rare cleverness in handling a phrase of singular nobility and beauty. In the *Capriccio* we admire ingenuity and spirit, humour and freshness of invention, it is a piece full of caprice and obstinacy but also of gentleness and amiability.

No. 3.—JOSEPH RHEINBERGER

(Born March 17, 1839, at Vaduz, Lichtenstein).

Toccata, op. 12.

This composer is an exceedingly clever, thoroughly instructed, and experienced artist; the chief qualities of his works are a natural and fresh healthy expression, an agreeable unpretentiousness, and decided strength. The *Toccata* is a truly excellent and spirited composition, full of fire, variety, and enthusiasm.

No. 4.—PETER TSCHAIKOWSKY

(Born December 25, 1840, at Wotkinski, Government of Perm).

(a) *Chanson sans paroles* in F.
(b) *Troïka*, in E.

This composer represents the national Russian element; his works surprise us by their startling changes of harmony, while their fresh, pulsating rhythmical life offers a decided charm, and the construction of his melodies excites our interest by their quaintness and simplicity.

No. 5.—EDVARD GRIEG

(Born June 15, 1843, at Bergen, Norway).

(a) "*On the Mountains.*"
(b) "*Norwegian Bridal Procession passing by.*"

E. Grieg's pianoforte-pieces take hold at once of the attention and favour of the amateurs; their striking originality and individuality cannot fail to excite interest; his works are eminently influenced by Norwegian melodies, and thus they represent in a most pleasant manner Norwegian life and character. The first piece possesses a rugged, sturdy, healthy force and vigour; the second may be justly considered as one of the most delicious musical genre-pictures.

No. 6.—XAVER SCHARWENKA

(Born January 6, 1850, at Samter, Posen).

Polish Dances, op. 3.

In Scharwenka we possess one of the most richly gifted composers of the present time; his melodies are fresh, genuine, and well constructed, his harmonies bold and vigorous, and the modulations natural and nowhere forced; in his rhythmical expression he is generally happy, and offers a good deal of variety. Scharwenka has been reproached for writing his Polish dances in the style of Chopin, but in as far as a national Polish dance, like the Mazurka (Masur, Mazurek), possesses certain rhythmical and harmonious features, which *make* it actually the Polish dance and not a Russian or German dance, and in as far as Chopin has repeatedly employed these features in his Mazurkas, there is no reason why another composer—also writing Mazurkas—should not employ the same means. The difference between Chopin's and Scharwenka's Polish dances consists in the first having a rather feminine, plaintive, and melancholy expression, whilst the latter (Scharwenka's) are expressive of manly vigour, chivalrous energy, and decided firmness.

No. 7.—Jean Louis Nicodé

(Born August 12, 1853, at Jerezik, near Posen).

(a) *Canzonetta.*
(b) *Tarentella.*

Nicodé's pieces (only published since a few years) struck at once a sympathetic chord in the hearts of the public, for some of his shorter pieces already enjoy a great, genuine, and well-deserved popularity. He is peculiarly happy in his modulations, and his harmonies are singularly euphonious. The "*Canzonetta*" is a genre-piece of a remarkably happy conception and of most refined workmanship. The "*Tarentella*" introduces a national Neapolitan air, which lends great charm to it, and produces a decided effect.

No. 8.—Moritz Moszkowski

(Born August 23, 1854, at Breslau).

(a) *Germany* } From the Suite "From Foreign Parts," op. 23.
(b) *Hungary* }

The great beauty and the delightful freshness and rhythmical charm of Moszkowski's Spanish Dances, op. 12, Album espagnol, op. 21, and the Suite "From Foreign Parts," op. 23, at once caught the public ear, and found their way into the libraries of British amateurs. Moszkowski is undoubtedly a highly talented composer, and is able to invent beautiful melodies, which he understands to surround with exquisite harmonies.

[E. P.]

GENERAL MONTHLY MEETING,

Monday, February 2, 1885.

The Hon. Sir **William R. Grove**, M.A. D.C.L. F.R.S. Manager and Vice-President, in the Chair.

Arthur Edward Durham, Esq. F.R.C.S.
James Love, Esq. F.R.A.S. F.G.S. F.Z.S.

were elected Members of the Royal Institution.

With reference to the report of the Managers read at the previous meeting, a Resolution was unanimously passed authorizing the Sale of a part of the New Three per Cent. Consols belonging to and standing in the name of the Royal Institution of Great Britain, sufficient to raise a sum of not exceeding £2000 cash.

The Special Thanks of the Members were returned for the following donations to the Fund for the Promotion of Experimental Research:—

Warren de la Rue, Esq.	£100
Sir Frederick Bramwell	£50

The Special Thanks of the Members were returned to Miss **Julia Moore**, for her present, on behalf of her sister, the late Miss **Harriet Jane Moore**, M.R.I., of a fine copy of Dante's 'Divina Commedia,' printed in 1564; and also two Water-Colour Drawings of the Old Laboratory of the Royal Institution, including Portraits of Professor **Faraday**, and his Assistant, Mr. **Anderson**, painted by Miss H. J. **Moore** in 1852.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Palæontologia Indica: Series X. Vol. III. Part 5; Series XIV. Vol. I. Part 3, Fasc. 4. 4to. 1884.
Records, Vol. XVII. Part 4. 8vo. 1884.
The Secretary of State for India—Lists of the Antiquarian Remains in Madras. By R. Sewell. Vol. II. 4to. 1884.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Transunti. Vol. VIII. Fasc. 16. 4to. 1884.
Academy of Natural Sciences, Philadelphia—Proceedings, 1884, Part 2. 8vo.
Amsterdam Royal Society of Zoology—Bijdragen tot de Dierkunde. Afl. 10. 4to. 1884.
Tijdschrift voor de Dierkunde. Jaar. 5, Afl. I. and II. 8vo. 1884.

Antiquaries, Society of—Proceedings, Second Series, Vol. X. No. 1. 8vo. 1884.
Asiatic Society, Royal—Journal, Vol. XVII. Part 1. 8vo. 1885.
Asiatic Society of Bengal—Proceedings, Nos. 8, 9. 8vo. 1884.
Journal, Vol. LIII. Part 1, No. 2. 8vo. 1884.
Astronomical Society, Royal—Monthly Notices, Vol. XLV. Nos. 1, 2. 8vo. 1884.
Basel Naturforschende Gesellschaft—Verhandlungen, 7te Thiel, 2tes Heft. 1884.
Birmingham Philosophical Society—Proceedings, Vol. IV. Part 1. 8vo. 1884.
British Architects, Royal Institute of—Proceedings, 1884-5, Nos. 4, 5, 6, 7. 4to.
Chemical Society—Journal for December 1884, January 1885. 8vo.
Chief Signal Officer, U. S. Army—Professional Papers, No. 14. 4to. 1884.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. IV. Part 6. 8vo. 1884.
Dax: Société de Borda—Bulletins, 2e Serie Neuvième Année: Trimestre 4. 8vo. 1884.
Duka, Surgeon-Major Theodore, M.D. F.R.C.S. M.R.I. (the Author)—Remarks on the Life and Labours of A. Csoma de Körös. (Journ. Asiatic Soc. 1884.)
East India Association—Journal, Vol. XVII. No. 1. 8vo. 1885.
Editors—American Journal of Science for December 1884, January 1885. 8vo.
Analyst for December 1884, January 1885. 8vo.
Angler's Note Book, No. 5. 4to. 1884.
Athenæum for December 1884, January 1885. 4to.
Chemical News for December 1884, January 1885. 4to.
Eastward Ho! Vols. I. and II. No. 7. 8vo. 1884.
Engineer for December 1884, January 1885. fol.
Horological Journal for December 1884, January 1885. 8vo.
Iron for December 1884, January 1885. 4to.
Nature for December 1884, January 1885. 4to.
Revue Scientifique and Revue Politique et Littéraire for December 1884, January 1885. 4to.
Science Monthly, Illustrated, for December 1884, January 1885. 8vo.
Telegraphic Journal for December 1884, January 1885. 8vo.
Franklin Institute—Journal, Nos. 708, 709. 8vo. 1884.
Fraser, Lieut.-Colonel A. T. R.E. M.R.I.—Atharva Veda Sanhita. Edited by Sewaklal Karsandâs. 8vo. Bombay, 1884.
Galloway, R. Esq. F.C.S. (the Author)—Reforms in the Mode of Conducting Chemical Examinations. (Journal of Science, 1885.)
Geographical Society, Royal—Proceedings, New Series, Vol. VI. No. 12; Vol. VII. No. 1. 8vo. 1884-5.
Geological and Natural History Survey of Canada—Vocabularies of the Indian Tribes of British Columbia. By W. F. Tolmie and G. M. Dawson. With Maps. 8vo. 1884.
Descriptive Sketch of the Physical Geography and Geology of Canada. By A. R. C. Selwyn and G. M. Dawson. 8vo. 1884.
Goldsmid, Lady—Memoir of Sir Francis Henry Goldsmid. 2nd edition. 8vo. 1882.
Hamilton Association—Journal and Proceedings, Vol. I. Part 1. 8vo. 1884.
Iron and Steel Institute—Journal for 1884, No. 2. 8vo.
Johns Hopkins University—American Chemical Journal, Vol. VI. Nos. 4, 5. 8vo. 1884.
American Journal of Philology, No. 19. 8vo. 1884.
Kew Observatory—Report, 1884. 8vo.
Linnean Society—Journal, Nos. 106, 135. 8vo. 1884. .
Macnaught, Rev. John, M.A. M.R.I. (the Author)—The Confessional in the Mission. A Lecture. 8vo. 1885.
Manchester Geological Society—Transactions, Vol. XVIII. Part 3. 8vo. 1885.
Mechanical Engineers' Institution—Proceedings, No. 4. 8vo. 1884.
Mensbrugghe, M. G. Van der (the Author)—Notice Biographique de Joseph A. F. Plateau, Hon. *M.R.I.* 12mo. Bruxelles, 1884.

Meteorological Office—Monthly Weather Reports for October, 1884. 4to.
Quarterly Weather Reports for 1877, Part 1. 4to. 1884.
Meteorological Society, Royal—Quarterly Journal, No. 52. 8vo. 1884.
Meteorological Record, No. 14. 8vo. 1884.
Michael, William H. Esq. Q.C. M.R.I. and J. S. Will, Esq. Q.C. (the Authors)—The Law relating to Gas and Water. 3rd edition. By M. J. Michael. 8vo. 1884.
Newton, A. V. Esq. (the Author)—Analysis of the Patent and Copyright Laws. 8vo. 1884.
Numismatic Society—Chronicle and Journal, 1884, Part 3. 8vo.
Odontological Society of Great Britain—Transactions, Vol. XVII. Nos. 2, 3. New Series. 8vo. 1884-5.
Pharmaceutical Society of Great Britain—Journal, December 1884, and January 1885. 8vo.
Calendar for 1885. 8vo.
Photographic Society—Journal, New Series, Vol. IX. Nos. 2, 3. 8vo. 1884.
Physical Society of London—Proceedings, Vol. VI. Part 3. 8vo. 1884.
The Scientific Papers of James Prescott Joule. Vol. I. 8vo. 1884.
Pollock, Frederick, Esq. M.A. LL.D. (the Editor)—Law Quarterly Review, Vol. I. No. 1. 8vo. 1885.
Richardson, B. W. M.D. F.R.S. (the Author)—The Asclepiad. Vol. II. No. 5. 8vo. 1885.
Rio de Janeiro, Observatoire Impérial de—Annales, Tome II. 4to. 1884.
Rogers, G. A. Esq. (the Author)—The Art of Wood Carving. 8vo. 1879.
Royal Society of London—Proceedings, No. 234. 8vo. 1884.
Philosophical Transactions, Vol. CLXXIV. Part 3; Vol. CLXXV. Part 1. 4to. 1883-4.
Sanitary Institute of Great Britain—Transactions, Vol. V. 8vo. 1884.
Saxon Society of Sciences, Royal—Philologisch-Historische Classe:
Abhandlungen: Band IX. Nos. 2-6. 4to. 1884.
Berichte, 1883. 8vo. 1884.
Mathematische-Physische Classe:
Abhandlungen: Band XIII. No. 1. 4to. 1884.
Berichte, 1883. 8vo. 1884.
Scottish Society of Arts, Royal—Transactions, Vol. XI. Part 2. 8vo. 1884.
Society of Arts—Journal, December 1884, and January 1885. 8vo.
Indexes to Journal, Vols. XI. to XXX. 8vo. 1873 and 1884.
Statistical Society—Journal, Vol. XLVII. Part 5, Appendix. 8vo. 1884.
St. Bartholomew's Hospital—Statistical Tables, 1883. 8vo. 1884.
St. Pétersbourg, Académie des Sciences—Mémoires, Tome XXXII. Nos. 4-12. 4to. 1884.
Telegraph Engineers, Society of—Journal, Vol. XIII. No. 54. 8vo. 1885.
Tokio University—Memoirs, No. 4. 8vo. 1884.
United Service Institution, Royal—Journal, No. 126. 8vo. 1884.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1884: Heft 9, 10. 4to.
Victoria Institute—Journal, No. 71. 8vo. 1884.
Woolnough, C. W. Esq. (the Author)—The whole Art of Marbling. 8vo. 1881.
Yorkshire Archæological and Topographical Association—Journal, Part 33. 8vo. 1885.

WEEKLY EVENING MEETING,

Friday, February 6, 1885.

Sir Frederick Bramwell, F.R.S. Manager and Vice-President,
in the Chair.

G. Johnstone Stoney, Esq. M.A. D.Sc. F.R.S.

How Thought presents itself in Nature.

All the main steps in the great progress which man has been able to make in understanding the universe in which we live and of which we form a part have consisted in ascertaining that there is more simplicity in what occurs than had before been known to prevail. It is the aim of the present discourse to endeavour to follow this simplification up to the stage to which scientific inquiry has now succeeded in carrying it, and to point out a further simplification, which is suggested and rendered probable by that which has been thus ascertained, although we are not yet in a position to speak confidently about this further step.

Sound is Motion.

A very important discovery was made when it was found that while we are listening to sound what is really occurring in the outer world is merely a motion of materials all of which were there before. Thus in playing a piano the strings are made to vibrate by the performer; this sets up a quivering motion in the sounding-board corresponding to the motion of the strings, and the sounding-board in its turn transmits an undulation through the air, some very small part of which occurs within the outer passage of our ear. It here encounters the membrane which separates the outer from the middle chamber of the ear, and through it communicates the motion to the air of the middle chamber and to three delicate little bones which finally carry the motion forward to the inner chamber where the true auditory apparatus suspended in a liquid is operated upon by the vibratory motion, and in its turn acts upon the multitudinous filaments of the elaborate fringe in which our nerve of hearing terminates.

The whole of the phenomenon then so far as it exists in the *outer* world consists entirely of motions, and it becomes sound only at some stage beyond the portals of our ears. When this was found out, a very important discovery was made respecting the actual operations of nature. This discovery was followed up by investigations, through which a multitude of details have been brought to light, as, for example, that the pitch of the sound as we hear it depends on the periodic time of the waves that occur outside us, that the quality of the tone depends on the form of the wave, and so on. The character also of the undulation in different materials has been ascertained. For instance, in air it consists at each point of a rapid succession of little winds blowing alternately forwards and backwards. The length of the waves in various media, the rapidity of their advance, and a multitude of other particulars about them, have been carefully measured. It has thus been ascertained that in air the longest waves we can hear are about twelve or fourteen metres from each point of compression to the next, the shortest about a centimetre; that the length of the air waves produced by the middle E of the piano is about one metre, and so on.

LIGHT IS MOTION.

Another and a most important advance in our knowledge of nature was made when discovery after discovery confirmed the truth of the undulatory theory of light and radiant heat, and justified us in accepting it. These and discoveries in molecular physics certify to us that we see objects because periodic motions in their molecules generate an undulation around that pulsates in harmony with them. A very small portion of this great undulation obtains access to our eyes through the pupils, and produces a change in some substance or substances of the black pigment that lies under the retina at the back of the eye. What this change is we do not yet fully know, but it is probably a fugitive change of the nature of the more permanent chemical change which occurs in photography. Of whatever kind it is, the altered pigment gains for the time the power, through an intricate apparatus of rods, cones, and bulbs, of exciting the optic nerve to act upon the brain. However, we are not just at present engaged in following up the series of events through our organs of sense to the brain and mind, but are only inquiring what part of the series takes place in the *outer* world. This part is simply motions: anything else that is in the series of events known to us comes in at some subsequent stage.

WHAT A SCIENTIFIC EXPLANATION IS.

In order to keep our conceptions clear, it will be convenient to take a general survey here of what would constitute a complete explanation of a phenomenon from the scientific naturalist's point of view. We may select any phenomenon—for example the green colour we see when we look at the foliage of a particular plant, let us suppose of a geranium. The perception of green in certain situations, which is, so long as we are looking at the plant, a part of the great complex thought which we call our mind, is the known element of this inquiry—equally known before the investigation as after it. It may be called the *a* or known part of our problem; and, adopting the practice of mathematicians, we will assign the last letters of the alphabet *z*, *y*, *x*, and *w* to those unknown parts which the scientific man endeavours to ascertain. His first inquiry is as to what has occurred in the outer world. This in the case we have taken he finds to be motions with ascertainable periodic times within and between molecules of the geranium, followed up by an undulation around that has the same periodic time as those motions, a very small part of which reaches the black pigment of the eye through the pupil. This is the first or *z* part of a scientific explanation of the phenomenon. It is the explanation of what is going on in the outer world, and the *z* of the problem would be considered by the scientific man as having been fully discovered if these motions and all that relates to the laws under which they occur had been made out.

The next, or *y* part of the investigation, is to discover what change this *z* produces within our organs of sense on those parts of them which stand in a direct functional relation with the associated nerves. This, in the example we have taken, would consist in ascertaining—1st. What new substances appear in consequence of the (probably three) photographic effects which the incident light has on the pigmentum nigrum; 2nd. What effect these have upon the apparatus of rods and cones which keeps tapping against the pigmentum nigrum; 3rd. What is the connection between this last change and the excitation of the nerve. The next in order would be the *x* part of the inquiry. It would be an inquiry into the physical change which the presence of these substances produces in the retina and along the optic nerve. If all these, and all about them, were fully made out the *y* and *x* parts of the explanation would be complete. And, finally, if it could be discovered what physical change ensues in the brain itself the *w* part of the problem would be solved, and the whole scientific explanation would be then complete.

In order of time the *z* part of the phenomenon first occurs in the outer world. Some excessively small part of this gains access to our organs of sense and produces in them the *y* part of the phenomenon, which acting on the nerves, and developing in them the *x* part of the phenomenon, excites them in turn to make that stir within the brain which is the *w* part of the phenomenon, along with which the *a* part of the phenomenon presents itself and the perception of green at certain situations in space comes to form for the time a part of our mind.

We are, however, as yet only concerned with the *z* part of the phenomenon—that part which takes place in the outer world, and with which science has been up to the present able most fully to deal. As we have seen, it consists entirely in motions. It has been found possible to measure many of these motions in various ways; and, to give some notion of what the universe really is, I will state the results of some of these determinations.

METRETS.

Scientific measures are now made in metres and divisions of the metre. The metre itself is a few inches more than a yard long, and is divided into metrets, a convenient name for its decimal subdivisions, that is subdivisions each of which is the tenth part of the one before it in the series, and ten times the next after it. The decimetre, or tenth part of a metre, is the first of these metrets; it is about a hand-breadth. The next metret is the centimetre, the hundredth part of a metre, and is about a nail-breadth. The third-metret is the millimetre, about the distance across a small pin's head. The fourth-metret is the tenth of this, and is about the thickness of a sheet of paper. The fifth-metret is microscopical; it is intermediate in size between the diameters of the red and white disks that float in human blood. The tenth of this, the sixth-metret, would be a very small object in the microscope, and no microscope is able to show the seventh-metret, which is the next of the series. However the study of nature has obliged us to go farther than the microscope can penetrate, and leads us on to, at all events, the tenth and eleventh of this series of metrets. The tenth-metret is so small that a child during the years of its most vigorous growth is growing at the average rate of between thirty and forty of them every second, and the eleventh-metret is the tenth part of this again. This is about as far as the scientific examination of nature has as yet obliged us to go.

QUANTITATIVE DETERMINATIONS—LIGHT AND SOUND.

Now the waves of light as they travel between the objects we are looking at and our eye, are of various lengths, but all shorter than the sixth-metret. The shortest are about four and the longest about eight of the seventh-metrets, but none so long as ten seventh-metrets, which would make up the whole of a sixth-metret. They must therefore take rank with microscopic objects so small that they can only be seen with a tolerably high power. Small as they are, these tiny waves advance with extraordinary speed, travelling a distance of thirty quadrants of the earth in a second of time, meaning by a quadrant the distance along a meridian from the earth's equator to the pole, a distance which measures ten millions of metres. The vibratory motion at each point of space is transverse to the direction in which the waves are travelling, a kind of motion with which we are familiar in the waves that run along the surface of water. The range of this transverse motion is about a tenth-metret, the very minute quantity to which I drew attention a while ago; and the rapidity with which it is repeated varies with the colour of the light, and for green light has about its mean value: in a ray which produces this colour the oscillatory movement is repeated about as often every second as there are seconds in nineteen millions of years.

This short sketch will, I hope, give a picture that will suffice for our present purpose, of the kind of motion in the outer world that affects us through our organ of sight. It stands in very broad contrast to that great coarse kind of motion which acts upon us through our ears, and which for the middle E of the piano is repeated only 330 times in a second. The direction of motion also is different, for in the sound waves of air it is backwards and forwards along the direction in which the waves are propagated, while in light it is transverse to that direction. But the greatest difference is one to which I have not yet drawn attention, viz.: that the motions in sound and light belong to different orders of motion. To understand this, it will be necessary to examine first other motions that are going on in nature.

GASES.

We have gained more knowledge of the motions that go on in gases than in either solids or liquids. A gas consists of separate little missiles called its molecules, darting about in every conceivable direction. In gas so perfect as the air about us at the surface of the earth there is room enough between these molecules for each in its flight usually to pass several of its fellows before its motion is interfered with by its encountering any of them. When the encounter takes place the two molecules that come together grapple with one another in a peculiar way, sometimes exchanging part of their contents, and always shaking up the internal motions that go on within them both. After they get free from each other they dart off in new directions, to be again turned aside when they encounter other molecules. In this way the course of each is an irregular zig-zag, consisting of little straight pieces corresponding to the free paths of the molecule between its encounters. The rapidity of its flight is liable to change at each encounter: by some encounters it is increased, by others diminished, by a very exceptional encounter it may possibly be left the same as before. The length of each little free path will of course depend upon how long the little traveller chances to avoid its neighbours.

The details of the motions cannot be separately traced, but nevertheless a good deal of valuable information about them has been obtained and many useful averages have been determined. For example, in the air about us, at the pressure and temperature which prevail in this room, there are about a unit-eighteen, or 1 with eighteen 0's after it (1,000000.000000,000000, a million times a million millions) of these molecules in each cubic millimetre, i. e. in about the volume of a pin's head. If at any instant they could be kept from darting about, and if their distances asunder were then measured, the average of all these various little distances would be about a ninth-metret, about as much as a child grows in the third of a second. They dash about at various rates, some more others less than 500 metres per second, but on the average at about that speed. This is about the quickest speed with which modern ordnance can launch their projectiles. It is not far short of a third of a mile in a second.

Of the available energy that is in the gas somewhat less than two-thirds takes the form of this great activity; the rest, which as more than one-third, is occupied in maintaining internal motions that go on *within* the molecules of the gas. About these internal motions we have much information given to us through observations made with the spectroscope, but it comes to us in an exceedingly fragmentary form, and it is difficult to get definite information out of it. Perhaps the most precise information we as yet possess is in the case of a periodic motion which occurs within the molecules of a vapour called chlorochromic anhydride, which motion has been found to be repeated about 809,520,000000 of times every second, and to be of such a kind as to bear a close analogy to the motion of a point on a violin string which is nearly but not quite two-fifths of the length of the string from one end.

To return to the air about us, in which the molecules are at about a ninth-metret asunder: it is only when they happen to get much nearer together than this ninth-metret that they can interfere with each other's motions, and the mean length of their free paths, the average distance to which they are able to make their way between their encounters is somewhere about three-quarters of a seventh-metret; from which and from the speed with which they are moving it is computed that each molecule usually is subjected to about 7000,000000 encounters in each second.

Accordingly we may get some picture of the path pursued by a molecule within one second of time by imagining a line 500 metres long (the length of Grosvenor Place or Portland Place, or twice the length of Albemarle Street) crumpled up till it has 7000 millions of angles upon it; the little straight bits will be of various lengths, all small, and on the average about three-quarters of a seventh-metret long, i. e. about one-third of the diameter of the smallest spec that can be seen with the best microscope.

But I do not know of a contrivance by which we can form any adequate conception of the far more dainty little motions *within* the molecules, upon which one-third of their energy is expended.

LIQUIDS AND SOLIDS.

When the gas is condensed into a liquid or frozen into a solid, the molecules are forced close together. In the liquid state they can still jostle about among one another though there is not room for free paths: in fact they are always in a state of encounter. And if the liquid is frozen into a solid the molecules become so much restrained that they cannot get past each other and are unable to travel away from the place assigned to them. However, the delicate and almost immeasurably rapid internal motions still go on, either in the same form as before, or, as more usually happens, in a modified form. It is these which start vibrations around, the waves of light and radiant heat, with various periodic times, the swiftest that have been measured by the spectroscope in the ultra-violet rays being repeated about as often each second as there are seconds in 50 millions of years, the slowest that are visible to our eyes being repeated as often as there are seconds in 12 millions of years, and still slower ones affecting us as heat though we have not such substances in the pigment of our eyes as would be influenced by them as light.

PRIMARY AND SECONDARY MOTIONS.

This will be a convenient place in which to draw attention to a distinction of importance in the study of nature: the radical distinction which exists between primary motions and secondary. Secondary motion exists wherever there is a transference from place to place of other underlying motions. Primary motions are those that have no other more subtile motions underlying them. None of the senses with which man is furnished, even when aided to the utmost by the microscope, have ever reached primary motions, nor have they indeed been able to penetrate beyond coarse forms of secondary motion; and it should be observed that the science of dynamics deals exclusively with this class of motions.

When the spore of a mushroom is hurried along by the wind, the vast accumulation of molecular motions that are going on within its small volume is being drifted forwards, and constitutes the visible motion of the spore a secondary motion. In what enormous numbers underlying motions are here present may be judged from considering that complex motions go on within every chemical atom, that there are within the most minute organic spec that has ever been seen with a microscope enough of chemical atoms to make up from 10,000 to 100,000 of the most complex organic molecules known to chemists, allowing thousands of chemical atoms for each of these complex molecules, and that a vast number of such organic specs would be needed to make up a very tiny spore. It will thus be perceived that the motions that go on within a spore are in inconceivable numbers and of extraordinary complexity. The motion of a spore, then, when drifted by the wind is a rather coarse secondary motion, i.e. it is the travelling forwards of a complicated mass of underlying motions.

A far less coarse motion is taking place in the molecules of gas as they dash about among each other in the way described above. Yet here again the motion is secondary, for within each molecule there are the motions going on that originate the lines seen with the spectroscope. Whether these internal motions are primary or secondary we do not yet know, but the probability seems to be that even they are secondary and that underlying them there are still more subtile motions. There is in fact some reason to suspect that chemical atoms may be likened to the vortex rings of which an imperfect illustration is shown in the beautiful experiment in which smoke rings are shot across a room. Two or more chemical atoms unite to form a molecule of the gas. The advance of the vortex ring across the room will stand for the travelling motion which carries an atom of the gas along as part of its molecule, the pulsation which is often seen passing round the vortex ring may represent the heat and light vibrations which take place within each atom of the molecule, and underlying both of these is the vortex motion itself, which may perhaps correspond to the real primary motion in the molecule. However, in the present state of science we do not know certainly that these last underlying motions exist, though we have reason to suspect them. If they do exist, then the heat motions are secondary; if they do not exist, then the heat motions are the primary motions.

A similar doubt exists in regard to the undulations of which light and radiant heat consist. In the present state of our knowledge it continues undecided whether they are primary motions, or whether they are waves transmitted through still more subtile motions underlying them, and if this be the case then they are secondary motions. But whether primary or secondary these etherial motions are manifestly of quite a different order from the motions which take place in sound. They are to be classed along with the subtile internal motions that go on *within* the molecules of bodies, and are very different from that general shifting backwards and forwards of vast accumulations of molecules which we hear as sound. This is that fundamental difference between light and sound of which I spoke earlier in this discourse.

EXTERNAL SOURCE OF OUR OTHER SENSATIONS.

When we extend this scrutiny to those events in the outer world which act on us through our other senses, we find motions throughout the material universe, motions everywhere, motions underlying every phenomenon; and no phenomenon has yet been met with in the material universe that when adequately examined does not resolve itself entirely into motions and the relations between motions. If a chemist comes across an object surrounded by those light waves which would give it to his eyes the appearance of being of the steel grey of sodium; if on approaching a knife to it he finds he can divide it, and that the molecular motions of the edge of his knife are made to retreat inwards to the small extent which causes him to feel the amount of resistance which sodium offers; if he allows it to drop and finds that it moves towards the earth as ponderable matter does; if he places it in the pan of his balance and finds that the upper molecular motions of the pan approach those beneath in the degree which requires a counterpoise corresponding to the weight of sodium to be moved into the other scale to prevent the rest of the pan from descending; if he sees the motions of combustion arise when he throws it on warm water, and ascertains by his spectroscope that the motion ensues in the surrounding space which occasions the characteristic lines of sodium; if on applying some of its salts to his tongue those motions occur in that organ which result in his perceiving a saline taste; if on approaching chemical tests to it he finds all the alterations of molecular motion arise which are manifested when a compound of sodium is produced: would any chemist hesitate one moment to pronounce that the object with which he has been dealing is sodium? Yet at every step he has done nothing but move objects about, and it has been motions and *nothing but motions and their changes* which he has really observed.

WHAT FORCE IS.

Many persons suppose that force, mass, energy and so on have an independent existence in nature. In reality they are functions of the motions that occur in nature, and of relations between these motions. These particular functions are so constantly presenting themselves in our dynamical calculations, that it is most convenient to have definite names and symbols for them, but it is a grave error to mistake them for the real agents, and not to recognise that they really are merely contrivances, most useful contrivances, for simplifying the description of complex phenomena, so as to enable us to grasp sufficiently those parts of a problem that are essential for the purpose we may happen to have in view.

It is sometimes said—"Force is the cause, motion the effect." This statement needs much amendment. Force is not any existing thing. When we speak of so much force as acting in any particular case, we only mean to indicate that *some* sufficient cause of acceleration is present acting up to that particular degree which is capable of producing a change of motions of a specified amount; and the convenience of the term is that it successfully *avoids* the necessity of paying attention to what the cause is.

The reason why a stone falls is not, as is sometimes supposed, that an agent which can be called the force of gravitation is acting upon it. The real cause is that the great mass of molecular motions called the earth is in sufficient proximity to that other mass of motions called the stone. The cause why a bolt is projected from a cross-bow is that by the action of the bow the molecular motions on the front of the string have been brought into sufficiently close relation to the molecular motions at the back of the bolt. The word force expresses that one or other of these causes, or some other cause capable of producing the effect, is present; with the great convenience that it relieves us from the necessity of determining which of the innumerable possible causes is the one acting in the case we are dealing with—a problem unnecessary, and often too difficult for solution. The term and its mathematical symbol are therefore most useful by enabling us to get on with our work, when we want to find out *how much effect* will arise, and do not care whether the real agent is a cord pulling, a spring urging, a magnet attracting, or any other. But in every case treated of in the science of dynamics (i. e. in every case in which we are studying the drifting about of masses of molecular motions), the real cause is that certain motions (whether that vast accumulation of motions called the earth, the end motions of the cord, the front motions of the spring, or whatever they may be) have come into the requisite position with relation to that other aggregation of motions that is moved.

WHAT MASS IS.

Similarly the symbol used by mathematicians and called by them the mass of an object, suppose of a stone, is the convenient contrivance by which they take notice of the fact that in every such case the drifting motion of both the bodies is affected, both of the body acting on the stone and of the stone. What is called the mass of the stone is really the measure of the acceleration received by the other body while it and the stone are acting on each other, so that the ratio of the two masses is the ratio of the accelerations taken in reverse order; or if we wish to make the statement general, the mass of the stone is the measure of all the changes of drifting motions going on elsewhere throughout the material universe which have relation to the simultaneously existing motions which we call the stone. The real cause that is operating is the proximity of the molecular motions in the stone to the molecular motions of some other body; one part of the effect is a change in the molar motions of both bodies, i. e. in the drifting about of their masses of molecular motions. Other effects also arise, but this is the part of the effects which the science of dynamics investigates. In fact dynamics does not concern itself in the least with the motions going on within the molecules of the bodies with which it is dealing. It is the science of the transference from place to place of underlying motions. The symbols for force and mass express *vaguely* the presence of adequate causes, while in terms of the effects these symbols are, in the science of dynamics, quite *definite*.

The proper description of the law of gravitation is that towards each particle of ponderable matter, i. e. towards the molecular motions within a small compact volume, all the other molecular motions that are going on in the universe are being drifted with accelerations that are inversely as the squares of their distances from that centre; and the ratio of the acceleration thus impressed by one particle on another when compared with the amount received by the first from the second is that ratio which is called the ratio of their masses. The resultants of these accelerations may properly be called *things*, in the sense that we find them really existing in nature: all the rest are contrivances of the mind to enable it sufficiently to grasp the phenomena.

OF SUBSTANCE.

Another prevalent impression is that, in addition to the motions we find existing in nature, there is a mysterious entity which is spoken of under the name of substance. Now Science has never found, and therefore knows nothing of, any object devoid of internal motions such as that often supposed under this name, added to external nature by the mind, not found by it in nature. Whence, then, comes it that there is an almost universal persuasion that underlying every motion there must be some *thing* to be moved? We need not search far to find the answer to this question. It has arisen from experience, from abundant experience: from our experience and the experience of the whole series of our progenitors down from the dawn of such organised thought upon the earth as we possess. Neither we nor any member of this long series ever felt or saw a primary motion at all, nor even a molecular motion, although some of these are secondary. The only motions that until in recent times any man could ascertain to be motion were the *coarser* forms of secondary motion, and even still every motion that our senses are competent to perceive to be motion has a very considerable thing that is moved, namely, vast aggregations of subsidiary motions, of kinds which science can show to be present, which we can *conceive* as motions, but which we are unable to perceive except under forms so disguised that they seem stationary. Until the discoveries of science were made, masses of molecular motions, such as clubs, boats, stones, grains of dust, &c., were mistaken by everybody for objects that might be brought to absolute rest. This is the only class of objects that any one ever felt or saw in motion, and so there grew up the supposition that wherever there is motion there is something moving which might be brought to rest. This conclusion, however, went beyond what the experience really warranted. The conclusion it warrants is that wherever we men, with certain limited senses, can *perceive* that there is motion by the special senses that we happen to possess, whether unassisted or with the best aid we can obtain from microscopes or other appliances, there is always something moving. This more guarded statement is perfectly correct; for since a sufficient scientific investigation became possible, the very important discovery has been made that in all the cases covered by the correct statement, there is a vast accumulation of subsidiary motions being drifted along. Anything else which any one supposes to be present, is really not what he knows to exist, but what he imagines.

EXPERIMENTS CONSIST OF MOTIONS.

Every experiment man can make consists altogether of motions. A chemist pours one of his solutions upon another. This bringing of them together is a rough secondary motion. This is followed by another motion, the mixing of the two solutions till the molecules get into such close apposition that they can act on each other. The next stage is again motion, when atoms of the one are exchanged for atoms of the other, and new compounds are formed. So also when in their new positions the motions of which the atoms consist, or with which they twine amongst one another, are subjected by their new neighbours to new constraints and new influences, and the motions become altered—when perhaps a new colour appears, betraying the fact that a periodic time has changed. In fact, in every part of the process, nothing but motions has ever been traced.

Take another example. We magnetise a piece of steel and alter some of its internal motions, yet how completely has this changed some of its dynamical relations to several other bodies of the universe! So when we electrify a conductor. Here we have altered motions, probably motions in the surrounding dielectric, and we have by doing so profoundly changed the relations of the system to the rest of the universe. So again, when we spin a gyroscope contained within a box, and by this very coarse internal motion have done enough to alter the inertia of the system, i. e. the way it behaves under impressed forces; and that not merely as respects its quantity but its kind. By an equally rough contrivance we can create by motions a property resembling elasticity; and so in other cases.

CONCLUSIONS—I. THE EXTERNAL SOURCE OF SENSATION IS MOTION.

The general outcome of all such inquiries is that we are confronted with the fact revealed to us by science, that every phenomenon of the outer world which we can perceive by any of our senses, is simply a mass of motions. The *z* of our inquiry is entirely motion.

II. SO ALSO IS WHAT OCCURS WITHIN OUR BODIES.

So also is the *y*, which means that part of the phenomenon which although still outside the nerves, is in more immediate relation to them. Nerves are not directly acted upon by what goes on outside our organs of sense, but by the change within our organs of sense which the external events occasion. For example, the molecular motions that are going on in the object we look at, do not reach our optic nerve. What they do is to produce luminous undulation around that body. Nor is it even this undulation, the light, that acts on our optic nerve. What it does is to make a change in the black pigment of the eye, and it is the new chemical substances thus evolved which are what act upon an apparatus of rods, cones, and bulbs in such a way that the latter become able to operate on the nerve. The new series of events that occur within the eye would, if fully understood, constitute the *y* part of the scientific naturalist's explanation; and although the details are imperfectly understood, they are all of those kinds that consist of changes of motion.

So again of the next, or *x* part of the scientific explanation. This is what occurs within the nerve fibres while they are receiving the impression from without, and exercising the power to act upon the brain which that stimulus has conferred upon them. Much is not yet known about what occurs within the nerve, but in consequence of the processes that go on, the nerve wastes and requires nourishment; it undergoes change, and the change which it undergoes advances inwards towards the brain at a rate that can be measured, and which is about the speed of the fastest railway train, a moderate speed compared with many others we meet with in nature. Here all that has been made out betokens that what occurs within the nerves is entirely made up of motions.

And, finally, the same is true of the brain itself. The motions that go on within that most wonderful organ are probably the most intricate that are known anywhere to prevail. But though unfortunately very little is understood about the operations that are going forward, there is nothing to raise the most distant suspicion that if an adequate examination could be made, the scientific naturalist would find any change going on in my brain while I am thinking that is not of those kinds which on ultimate analysis resolve themselves into motions and the relations between motions.

THE UNIVERSE ONE.

And here, after our survey of the material universe let us pause for an instant to reflect what all this really means. Some of the motions that are going forward in nature we call ponderable matter, others we call the ether; and again, within the range of ponderable matter we think of individual bodies each separate from the others. Now all this is very convenient, not only convenient but indeed essential to our making an intelligent use of the great world that lies around us—but it is not an accurate presentation of what really takes place. The motion that pervades the universe is not many motions but one motion, indescribably complex, but not divisible into parts that exist separate from one another. All the parts, if they can be called parts, mutually interpenetrate each other; and we do not view them correctly when we think of any one of them as having an existence independent of the rest. The motions that went on yesterday in the table at which I stand are succeeded by those that are going on there now, but these are not the only offspring of those former motions, neither are those the sole progenitors of these present motions. Those motions of yesterday also gave rise to an undulation around, some of which having escaped through the skylight is at this moment urging its rapid flight over an ever widening area to the distant stars; other parts of the undulation enabled this table to be seen by every member of the audience which was in this room yesterday; much of it fell upon the walls, the ceiling, the floor, bearing to them heat, light, electricity, which have made their motions, the motions in the rooms beyond them, in the street, all over London, different now from what they otherwise would have been. The earth, as it darted forwards on its course has borne all the molecular motions of that table along with it, and changed their distance and direction from the other bodies of the universe: and, as a consequence, the sun himself, the planets, the most distant stars have recognised its influence, have ceased to bend towards its former position, and are at this moment inclining a little towards that it now occupies. All these motions and multitudes of others are the lineal descendants of the molecular motions which yesterday constituted this table; and, correspondingly, those which are now going on in the table have not wholly sprung from the motions in it yesterday, but the entire of the rest of the universe has contributed to them.

Many will be disposed to say, "These effects are trifling, too small to deserve notice." This is not so, though some are small, not one of them is one whit the less real; neither are they in their own nature small, and some of them are conspicuous even to the few and slender means which men possess of becoming acquainted with them. Bring even one candle into this room at night, and in an instant its small flame excites molecular motions in the walls and furniture, and rouses them so strenuously that they maintain a great complex motion filling the entire space, one ten-millionth part of which entering our eyes is enough to enable us to see them all. And when it so effectually sets up motions near the surfaces of those bodies, these in their turn react on motions beyond, and so none of the motions within those bodies are quite what they were before, nor indeed can any part of the whole universe remain unchanged. There is no one body about us which would be what it is, if any one of all the other bodies of the universe had been other than what it was. The universe then is one, and is not made up of parts that exist separately.

IN EACH PART OF WHICH THERE IS MATERIAL FOR INFORMATION ABOUT ALL THE REST.

This is one reflection to which our inquiries naturally give rise. Another, which is closely allied to it, is that in each part of this great universe there is information about all the rest, which only requires an adequate interpreter to be brought conspicuously out. Such an interpreter, organised to deal with one small part of the information really contained in the motions that are dealt with, is our eye, with our optic nerve, our brain and our mind behind it. And what are the motions that it analyses? They are a small selection from those going on in the little circular disk of space which lies beyond the eye immediately in front of its cornea, with a diameter equal to the pupil of the eye and which may be very thin. When we stand in the country and see every blade of grass beneath our feet, birds and insects on the wing, the leaves upon a thousand trees, clouds, mountains, hedges and fields—what is really occurring is that molecular motions in all those objects have been brought to such activity by the undulation which has spread towards them from the sun, that they in turn have been able to send abroad motions in all directions, some of which have reached the tiny patch described above in front of each of our eyes. None of all that motion beyond conveys information to us except by being the cause why those two tiny patches are moving as they are; and the meaning of a small part of the motion that has been thus excited within those two little disks is in one particular way interpreted for us by the eye, and contains within it all the information given to us through that organ, and indeed contains vastly more. An equal amount of information about what is going on elsewhere is contained within every similar patch of space throughout the whole universe.

OUTCOME OF THE SCIENTIFIC INQUIRY.

Thus we may extend the statement made before, and say that scientific inquiry finds motion pervading the material universe; motion everywhere, motions underlying every phenomenon, and it finds *nothing existing outside the mind excepting motions.*

OUTCOME OF A FURTHER INQUIRY.

Here we might rest and be content with the assurance that there are at most only two kinds of existence known to us, thought and motion—all that is within the mind being thought, all that is without it motion. To this conclusion science inevitably leads; and of its truth, as of all other scientific conclusions, reasonable minds with a sufficient training can fully satisfy themselves. However, it is not possible to be content to stop here, and the further step to which we feel almost impelled, unfortunately falls not within the domain of science but of metaphysics. I say "unfortunately," because the reasoning which is at our disposal in metaphysics, unlike that of science, is in reality not an appeal to every trained human mind of sufficient capacity, but to those which have a leaning towards one particular school of thought. That this is the case we are forced to recognise, when in metaphysics we find able and well-instructed minds taking opposite sides, even where their opposition is as great as it is between Realists and Idealists.

What then is that motion to which science reduces every material phenomenon? The *conception* of motion is a thought within our minds. This conception exists, but it exists internally; it has no non-egoistic or external existence. It however has its source in part in something existing outside us. This something (in any particular instance) acts upon something else, that upon something further, that again upon another something, and so on, till, at the end of a long series of this kind, the last of these somethings is a sensation or sensations which form part of our mind for the time being. These sensations are worked up with what else is then in the mind in such a way that the general outcome of the whole is what we call the perception or conception of a motion. But in reality all we know of the original external source is that it is at the farther end of this long series of causes and effects, and that thoughts of which we are conscious, and which therefore we really know, are at the nearer end. Though we have spoken of intermediate steps of the series, of the y, x, and w steps, of chemical or other material changes that occur within our organs of sense, nerves, and brain, the meaning of what we have said is only this, that if what is here going on *were placed at the farthest end of such a series*, if in fact made a z and examined as such by some human mind through *its* organs of sense, nerves, and brain, that *under these circumstances* what is going on would rouse *in that mind* sensations which science has succeeded in referring to motions as the z part of the cause. But it is only if reaching the mind by that special circuitous course that motions, whether inside or outside our bodies, are known to elicit sensations. What they are in themselves we know not; what their effect within the mind would be if they approached it in any other way we know not; we only know that they produce, with uniform certainty, this particular series of events when operating in this particular way. We ought therefore to be in readiness to accept any evidence as to what they really are, for in all we know there is nothing to be set against that evidence. Now there is some evidence, and even considerable evidence, that that external cause is thought, not our thought, but thought that is going on elsewhere than in our consciousness. For this hypothesis, the simplest that can be entertained, quite gets rid of what is else an oppressive difficulty, the abrupt appearance of thought along with that one particular organisation which we call a brain, although the whole train of physical causes and effects *is complete without it, and leaves no room for it.* On the hypothesis now put forward the thought which is associated with a brain would be no "Jack in the box," springing up suddenly before us, but would be in full consonance with *the ordinary course of nature.* The weight of these considerations will be found to be very considerable when they are carefully pondered over, and I think warrant us—

1st. In being *confident* that all existence known to us is at most of two kinds, thought and whatever is the external source of our conceptions of motion; and

2nd. In regarding it as *probable* that the latter existence is thought — not our thoughts, but the thought of the great "Anima," or rather Animus "Mundi."

OTHER CONSIDERATIONS SUPPORT THIS CONCLUSION.

Every simplification of nature which can be effected recommends itself to our scientific judgment, and we are justified in esteeming very highly the evidence upon which we may carry the simplification of nature to this farthest point. What we have already adduced is supported by other considerations. The hypothesis calls upon us to admit that the thoughts of which we are conscious as our mind, and are aware of as minds in our fellow-men and in other animals are in reality very small swirls in an illimitable ocean of thought. Moreover, our mind, the cerebration of which we are conscious, can be but a small portion even of the thought going on inside our own brain, the rest being as much outside our consciousness as are the thoughts of our fellow-men. We may judge how much lies beyond our consciousness by reflecting that we have no thoughts at all with such time relations as the original molecular motions of our brains, so that all these, though they are present, lie utterly remote from the grasp of such a consciousness as ours. Our consciousness is in fact very much more restricted on this side. Every one must have noticed that as we become expert in any art, in walking, speaking, reading, playing from music, riding a bicycle, it comes to be done more and more outside our consciousness, and partly because the separate acts of the process then succeed one another too promptly to be consciously followed. While the habit is being formed it is found to hang for a long time on the boundary between consciousness and unconsciousness, passing inside that boundary when we give special attention to what we are doing, passing outside when we otherwise occupy our thoughts.

This is intimately connected with another shortcoming of our minds, that the quantity of thought which can come within our consciousness at any one time is limited. We find that by what we call attending we can exercise a choice amid such suitable materials as are present within the brain; but that by choosing some we exclude others. In a large assembly a babel of sounds may reach our brain: by attention we can bring more emphatically within our consciousness some of these sounds, and thereupon the rest pass partially beyond consciousness. Before we make our choice we hear a confused and much louder noise. After we set ourself to listen to one speaker out of many, to the rustle of a particular silk dress, or to any other of the tangle of sounds that assail us, those we have directed our attention to become more distinctly heard, the rest less heard, those we have selected are brought far within the boundary of consciousness and will be long remembered, the others pass partly beyond and if heard at all are soon forgotten—indeed they may pass entirely beyond our consciousness and be wholly unnoticed, if our attention to the selected sounds is sufficiently intense.

A very curious phenomenon closely related to the foregoing occurs when the clock strikes while we are so preoccupied, that though we hear that it is striking, we do not know what o'clock has struck. If just after it has struck, "with the sound still ringing in our ears," we wish to know the hour, it is generally possible to go over in memory what has recently been heard and to count the strokes, the number of which was until then unknown.

We often avail ourselves of these properties of our minds. The way in which we dismiss a thought from our mind is by vigorously devoting our attention to something else. It is in fact the only way we have of doing so.

By carefully observing matters of this kind we may satisfy ourselves that the thought of which we are conscious is a portion of a larger body of thought of the same kind lying close to our consciousness, in fact within our brain. The considerations brought forward in the earlier part of this discourse, give us reason strongly to suspect that there is also present within the brain a vastly more extensive body of thought differing, at all events in time relations, from any thought of which we can be conscious; and further that the thought that is present in the brain, whether the little that we are conscious of or the rest, is but one drop of a boundless ocean.

Here I must end. I dwelt chiefly on the first part of my subject as being the most appropriate to be fully discussed in the hall in which we are here assembled, and this has left little time for expounding the second. If time had allowed, the second part would have led us through considerations of supreme interest, and its issue is to reveal to us a vision of the universe of unsurpassed sublimity.

[G. J. S.]

POSTSCRIPT.

It may be useful to give a recapitulation of the main steps of the argument.

A small part of what takes place in the outer world affects my senses, and through them indirectly affects me.

The result of a successful scrutiny of what is going on in the outer world may for convenience be called the z part of the scientific explanation of what I witness.

The result of a successful scrutiny of the change which the z occasions within my organs of sense would be the y part of the explanation.

The result of a successful scrutiny of the changes that ensue along the associated nerves would be the x part of the explanation.

And, finally, the result of a successful scrutiny of the physical changes which this x produces within my brain would be the w part of the explanation; and the explanation of the phenomenon from the naturalist's point of view would be then complete.

The state of my own mind while I am witnessing what is going forward consists of sensations, perceptions, and conceptions, or some combination of these with reflections, reminiscences, associations, feelings, emotions, and so on, and may be called the a part of the inquiry, as it is the known part, known as fully before as after the investigation.

The whole of a scientific inquiry starts with this a, and is concerned in discovering the z, the y, the x, and the w.

We now know enough of this class of investigation to assure us that the whole of the z explanation, the y explanation, the x and the w, would, if we could fully explore them, turn out to be motions, and the laws that govern changes of motion. That, for example, the geranium I am now looking at, is a mass of molecular motions (whether with, as many people suppose, or without, as I suppose, that 'something else' which they call substance); that the molecular motions in the geranium modify the ether motions going on around it in the room; that the etherial motions when thus altered produce new motions in the back of my eye; that this event next alters motions in the associated nerve; and, finally, that the disturbance in the nerve produces an effect upon the motions going on in my brain. Here, if all the details were ascertained, as to what particular motions are occurring at each step, and what laws regulate their occurrence, Natural Science would have said her whole say. It has, however, then to be added that although this series is self contained and complete, there occurs another event alongside of it without any traceable place in the sequence of causes and effects, viz. the change within my mind which I call perceiving the geranium. Herein lies the great difficulty of the dualistic hypothesis, from which the monistic hypothesis is free.

Motion throughout this statement has meant, not my perception or conception of motion, but the factor in the outer world whatever it is, which, co-operating with factors within my mind produces that perception or conception. It is this outside factor which there seems reason to suspect is thought going on beyond my consciousness. When put into the language of this hypothesis, the result of the scientific inquiry would be expressed thus: the geranium consists of eminently complex thoughts going on outside my consciousness, which are in fact a part of the great Animus Mundi. These act on other thoughts, commonly regarded as waves of light, which again alter part of that vast assemblage of thoughts called one of my organs of sense. These, in their turn, act on another group of thoughts called the associated nerve, and finally, the thoughts which constitute the nerve bring about a modification within that other vast assemblage of thoughts called my brain; out of which last assemblage a few small pinnacles rise within my consciousness, i. e. have a special interaction and include special relations to preceding states, and so become parts of *one complex thought*, my mind.

That there is one great consciousness extending through the whole Animus Mundi, is I think suggested by the known fact that each part of material nature acts on and is acted on by every other.

On so abrupt a statement of the drift of an hypothesis which lies outside men's usual highways of thought, it will necessarily seem fanciful; but just as what is at first sight plausible may prove to be not true, so what on a first view strikes us as fanciful may turn out to be sober fact.

[G. J. S.]

APPENDIX.

This Friday Evening discourse was accompanied by three afternoon lectures at the Royal Institution, in which the lecturer endeavoured to explain how some of the determinations referred to in the discourse had been effected. For accurate determinations of wave-lengths of light the reader may refer to Ångström's 'Spectre Normal du Soleil'; for the motions in gases to Maxwell's 'Heat,' pp. 297 and 299, and to a paper* by the lecturer on the internal motions of gases in the 'Philosophical Magazine' for August, 1868; for the motions in chlorochromic anhydride to a paper by the lecturer and Professor J. Emerson Reynolds in the 'Philosophical Magazine' for July 1871. Most of the other determinations are in the text-books.

---

* Readers of that paper are requested to change the square of 16 into the square-root of 16, at the end of the second paragraph.

WEEKLY EVENING MEETING,

Friday, February 13, 1885.

Sir Frederick Pollock, Bart. M.A. Manager and Vice-President, in the Chair.

Sir John Lubbock, Bart. M.P. D.C.L. F.R.S. *M.R.I.*

The Forms of Leaves.

Greatly as we all appreciate the exquisite loveliness of flowers, it must be admitted that the beauty of our woods and fields is quite as much due to the marvellous grace and infinite variety of foliage. How is this inexhaustible richness of forms to be accounted for? Does it result from an innate tendency of the leaves in each species to assume some particular shape? Has it been intentionally designed to delight the eyes of man? Or has it reference to the structure and organisation—the wants and requirements of the plant itself?

Size.

Now, if we consider firstly the size of the leaf we shall find that it stands in close relation to the thickness of the stem, and that when strict proportion is departed from the difference can generally be accounted for. This was shown, for instance, by a table giving the leaf area and the diameter of stem of the Hornbeam, Beech, Elm, Lime, Spanish Chestnut, Ash, Walnut, and Horse Chestnut.

The size, once determined, exercises much influence on the form. For instance, in the Beech the leaf has an area of about 3 square inches. The distance between the buds is about 1¼ inch, and the leaves lie in the general plane of the branch, which bends slightly at each internode. The basal half of the leaf fits the swell of the twig, while the upper half follows the edge of the leaf above; and the form of the inner edge, being thus determined, decides that of the outer one also. In the Lime the internodes are longer, and the leaf consequently broader. In the Spanish Chestnut the stem is nearly three times as stout as that of the Beech, and consequently can carry a larger leaf-surface. But the distances between the buds are often little greater than those in the Beech. This determines, then, the width, and, by compelling the leaf to lengthen itself, leads to the peculiar form which it assumes.

Arrangement.

Moreover, not only do the leaves on a single twig admirably fit one another, but they are also adapted to the ramification of the twigs themselves, and thus avail themselves of the light and air, as we can see by the shade they cast without large interspaces or much overlapping. In the Sycamores, Maples, and Horse Chestnuts, the arrangement is altogether different. The shoots are stiff and upright, with leaves placed at right angles to the plane of the branch, instead of being parallel to it. The leaves are in pairs, and decussate with one another, while the lower ones have long petioles, which bring them almost to the level of the upper pairs, the whole thus forming a beautiful dome.

For leaves arranged as in the Beech, the gentle swell at the base is admirably suited, but in a crown of leaves, such as those of the Sycamore, space would be thereby wasted, and it is better that they should expand at once, as soon as their stalks have carried them free from the upper and inner leaves; hence we see how beautifully the whole form of these leaves is adapted to the mode of growth and arrangement of the buds in the plants themselves.

In the Black Poplar the arrangement of the leaves is again quite different. The leaf-stalk is flattened from side to side, so that the leaves hang vertically. In connection with this it will be observed that while in most leaves the upper and under surfaces are quite unlike, in the Black Poplar, on the contrary, they are very similar. The stomata, or breathing holes, moreover, which in the leaves of most trees are confined to the under-surface, are in this species nearly equally numerous on both. The "Compass Plant" of the American prairies, a yellow Composite not unlike a small Sunflower, is another plant with upright leaves, which, growing in the wide open prairies, tend to point north and south, thus exposing both surfaces equally to the light and heat. It was shown by diagrams that this position also affected the internal structure of the leaf.

In the Yew the leaves are inserted close to one another, and are long and linear; while in the Box they are further apart and broader. In the Scotch Fir the leaves are linear, and 1½ inch long, while in other Pines, as, for instance, the Weymouth, the stem is thicker and the leaves longer.

In the plants hitherto mentioned, one main consideration appears to be the securing of as much light as possible; but in tropical countries the sun is often too powerful, and the leaves, far from courting, avoid the light. The typical Acacias have pinnate leaves, but in many Australian species the true leaves are replaced by a vertically flattened leaf-stalk. It will be found, however, that the seedlings have leaves of the form typical in the genus. Gradually the leaf becomes smaller and smaller, until nothing is left but the flattened leaf-stalk or phyllode. In one species the plant throughout life produces both leaves and phyllodes, which give it a very curious and interesting appearance. In Eucalyptus, again, the young plant has horizontal leaves, which in older ones are replaced by scimitar-shaped phyllodes. Hence the different appearance of the young and old trees which must have struck every visitor to Algiers or the Riviera.

Evergreens.

We have hitherto been considering mainly deciduous trees. In evergreens the conditions are in many respects different. It is generally said that leaves drop off in the autumn because they die. This, however, is not strictly correct. The fall of the leaf is a vital process, connected with a change in the cellular tissue at the base of the leaf-stalk. If the leaves are killed too soon they do not drop off. Sir John illustrated this by some twigs which he had purposely broken in the summer; below the fracture the leaves had been thrown off, above they still adhered, and so tightly that they could support a considerable weight. In evergreen trees the conditions are in many respects very different. It is generally supposed that the leaves last one complete year. Many of them, however, attain a much greater age; for instance, in the Scotch Fir, two or three years; in the Spruce and Silver, six or seven; in the Yew even longer. It appears from this that they require a tougher and more leathery texture. When we have an early fall of snow our deciduous trees are often much broken down; glossy trees have a tendency to throw it off, and thus escape; hence evergreen leaves are very generally smooth and glossy. Again, evergreen leaves often have special protection, either in an astringent or aromatic taste, which renders them more or less inedible; or by thorns and spines. Of this the Holly is a familiar illustration; and it was pointed out that in old plants, above the range of browsing quadrupeds, the leaves tend to lose their spines, and become unarmed. The hairs on leaves are another form of protection; on herbs, the presence of hairs is often associated with that of honey, as they protect the plants from the visits of creeping insects; hence perhaps the tendency of water species to become glabrous, *Polygonum amphibium* being a very interesting case, since it is hairy when growing on land, and smooth when in water. Sir John then dealt with cases in which one species mimics another, and exhibited a striking photograph of a group of Stinging Nettles and Dead Nettles, which were so much alike as to be hardly distinguishable. No one can doubt that the Stinging Nettle is protected by its poisonous hairs, and it is equally clear that the innocuous Dead Nettle must profit by its similarity to its dangerous neighbour. Other similar cases were cited.

He had already suggested one consideration which in certain cases determined the width of leaves, but there were others in which it was due to other causes, one being the attitude of the leaf itself.

In many genera with broad and narrow leaved species, Drosera and Plantago, for instance, the broad leaves formed a horizontal rosette, while the narrow ones were raised upwards. Fleshy leaves were principally found in hot and dry countries, where this peculiarity had the advantage of offering a smaller surface, and therefore exposing the plant less to the loss of water by evaporation.

Water Plants.

Many of the acquatic plants have two kind of leaves—one more or less rounded, which floats on the surface, and others cut up into narrow filaments, which remain below; the latter thus present a greater extent of surface. In air, however, such leaves would be unable to support even their own weight, much less to resist any force such as that of the wind. In perfectly still air, for the same reason, finely divided leaves may be an advantage, while in comparatively exposed situations more compact leaves may be more suitable. It was pointed out that finely cut leaves are common among low herbs, and that some families which among the low and herb-like species have such leaves, in shrubby or ligneous ones have leaves more or less like those of the Laurel or Beech.

An interesting part of the subject is connected with the light thrown by the leaves of seedlings. Thus the Furze has at first trifoliate leaves, which gradually pass into spines. This shows that the Furze is descended from ancestors which had trifoliate leaves, as so many of its congeners have now. Similarly in some species, which when mature have palmate leaves, those of the seedling are heart-shaped. Could it be possible that the palmate form was derived from the heart-shaped, and that when in any genus we find heart-shaped and lobed leaves, the former may represent the earlier or ancestral condition? He then pointed out that if there was some definite form told off for each species then surely a similar rule ought to hold good for each genus. The species of a genus might well differ more from one another than the varieties of any particular species; the generic type might be, so to say, less closely limited; but still there ought to be some type characteristic of the genus. He took then one genus, that of Senecio (the Groundsel). Now, in addition to Senecios more or less resembling the common Groundsel, there were species with leaves like the Daisy, bushy species with leaves like the Privet and the Box, small trees with leaves like the Laurel and the Poplar, climbing species like the Tamus and Bryony. In fact, the list is a very long one, and showed that there is no definite type of leaf, but that the form in the various species depends on the condition of the species. From these and other considerations he concluded that the form of leaves did not depend on any inherent tendency, but on the structure and organisation, the habits and requirements of the plant. Of course it might be that the present form had reference to former and not to present conditions. This rendered the problem all the more complex and difficult. The lecture was illustrated by numerous diagrams and specimens, and Sir John concluded by saying the subject presented a very wide and interesting field of study, for if he were correct in his contention every one of the almost infinite forms of leaves must have some cause and explanation.

[J. L.]

WEEKLY EVENING MEETING,

Friday, February 20, 1885.

Sir Frederick Bramwell, F.R.S. Manager and Vice-President, in the Chair.

William Huggins, Esq. D.C.L. LL.D. F.R.S. *M.R.I.*

On the Solar Corona.

If it were usual to prefix a motto to these evening discourses, I might have selected such words as "Seeing the Invisible," for I have to describe a method of investigation by which what is usually unseeable may become revealed. We live at the bottom of a deep ocean of air, and therefore every object outside the earth can be seen by us only as it looks when viewed through this great depth of air. Professor Langley has shown recently that the air mars, colours, distorts, and therefore misleads and cheats us to an extent much greater than was supposed. Langley considers that the light and heat absorbed and scattered by the air and the particles of matter floating in it amount to no less than 40 per cent. of the light falling upon it. In consequence of this want of transparency and of the presence of finely divided matter always more or less suspended in it, the air, when the sun shines upon it, becomes itself a source of light. This illuminated aerial ocean necessarily conceals from us by overpowering them any sources of light less brilliant than itself which are in the heavens beyond. From this cause the stars are invisible at midday. This illuminated air also conceals from us certain surroundings and appendages of the sun, which become visible on the very rare occasions when the moon coming between us and the sun cuts off the sun's light from the air where the eclipse is total, and so allows the observer to see the surroundings of the sun through the cone of unilluminated air which is in shadow. It is only when the aerial curtain of light is thus withdrawn that we can become spectators of what is taking place on the stage beyond. The magnificent scene never lasts more than a few minutes, for the moon passes and the curtain of light is again before us. On an average, once in two years this curtain of light is lifted for from three to six minutes. I need not say how difficult it is from these glimpses at long intervals even to guess at the plot of the drama which is being played out about the sun.

The purpose of this discourse is to describe a method by which it is possible to overcome the barrier presented to our view by the bright screen of air, and so watch from day to day the changing scenes taking place behind it in the sun's surroundings.

The object of our quest is to be found in the glory of radiant beams and bright streamers intersected by darker rifts which appears about the sun at a total solar eclipse. The corona possesses a structure of great complexity, which is the more puzzling in its intricate arrangement because though we seem to have a flat surface before us, it exists really in three dimensions. If we were dwellers in Flatland and the corona were a sort of glorified catherine-wheel, the task of interpretation would seem less difficult. But as we are looking at an object having thickness as well as extension, the forms seen in the corona must appear to us more or less modified by the effect of perspective. This consideration tells us also that the intrinsic brightness of the corona towards the sun's limb is much less than its apparent brightness as seen by us, of which no inconsiderable part must be due to the greater extent of corona in the line of sight as the sun is approached. The corona undergoes great and probably continual change, as the same coronal forms are not present at different eclipses.

The attempts which have been made from time to time to see the corona without an eclipse have been based mainly upon the hope that if the eye were protected from the intense direct light of the sun, and from all light other than that from the sky immediately about the sun, then the eye might become sufficiently sensitive to perceive the corona. These attempts have failed because it was not possible to place the artificial screen where the moon comes, outside our atmosphere, and so keep in shadow the part of the air through which the observer looks. The latest attempts have been made by Professor Langley at Mount Whitney, and Dr. Copeland, assistant to Lord Crawford, on the Andes. Professor Langley says, "I have tried visual methods under the most favourable circumstances, but with entire non-success." Dr. Copeland observed at Puno, at a height of 12,040 feet. He says: "It ought to be mentioned that the appearances produced by the illuminated atmosphere were often of the most tantalising description, giving again and again the impression that my efforts were about to be crowned with success."

There are occasions on which the existence of the brighter part of the corona near the sun's limb can be detected without an eclipse. The brightness of the sky near the sun's limb is due to two distinct factors, the air-glare and the corona behind it, which M. Janssen considers to be brighter than the full moon. When Venus comes between us and the sun, it is obvious that the planet as it approaches the sun, comes in before the corona, and shuts off the light which is due to it. To the observer the sky at the place where the planet is appears darker than the adjoining parts, that is to say, the withdrawal of the coronal light from behind has made a sensible diminution in the brightness of the sky. It follows that the part of the sky behind which the corona is situated must be brighter in a small degree than the adjoining parts, and it would perhaps not be too much to say that the corona would always be visible when the sky is clear, if our eyes were more sensitive to small differences of illumination of adjacent areas. My friend Mr. John Brett, A.R.A. tells me that he is able to see the corona in a telescope of low power.

The spectroscopic method by which the prominences can be seen fails because a part only of the coronal light is resolved by the prism into bright lines, and of these lines no one is sufficiently bright, and co-extensive with the corona, to enable us to see the corona by its light, as the prominences may be seen by the red, the blue, or the green line of hydrogen.

The corona sends to us light of three kinds. (1) Light which the prism resolves into bright lines, which has been emitted by luminous gas. (2) Light which gives a continuous spectrum, which has come from incandescent liquid or solid matter. (3) Reflected sunlight, which M. Janssen considers to form the fundamental part of the coronal light.

The problem to be solved was how to disentangle the coronal light from the air-glare mixed up with it, or in other words how to give such an advantage to the coronal light that it might hold its own sufficiently for our eyes to distinguish the corona from the bright sky.

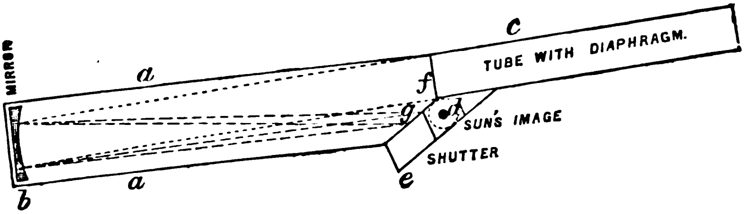
When the report reached this country in the summer of 1882 that photographs of the spectrum of the corona taken during the eclipse in Egypt showed that the coronal light seen from the earth, as a whole is strong in the violet region, it seemed to me probable that if by some method of selective absorption this kind of light were isolated, then when viewed by this kind of light alone the corona might be at a sufficient advantage relatively to the air-glare to become visible. Though this light falls within the range of vision, the eye is less sensitive to small differences of illumination near this limit of its power. This consideration and some others led me to look to photography for aid, for it is possible by certain technical methods to accentuate the extreme sensitiveness of a photographic plate for minute differences of illumination. [A cardboard on which a corona had been painted by so thin a wash of Chinese white that it was invisible to the audience, had been photographed. The photograph thrown upon the screen showed the corona plainly.] This cardboard represents the state of things in the sky about the sun. The painted corona is brighter than the cardboard, but our eyes are too dull to see it. In like manner the part of the sky near the sun where there is a background of corona, is brighter than the adjoining parts where there is no corona behind, but not in a degree sufficiently great for our eyes to detect the difference.

A photographic plate possesses another and enormous advantage over the eye, in that it is able to furnish a permanent record of the most complex forms from an instantaneous exposure.

In my earlier experiments the necessary isolation of violet light was obtained by interposing a screen of coloured glass or a cell containing potassic permanganate. The possible coming of false light upon the sensitive plate from the glass sides of the cell, as well as from precipitation due to the decomposition of the potassic permanganate under the sun's light, led me to seek to obtain the necessary light-selection in the film itself. Captain Abney had shown that argentic bromide, iodide, and chloride, differ greatly in the kind of light to which they are most sensitive. The chloride is most strongly affected by violet light from *h* to a little beyond K. It was found possible by making use of this selective action of argentic chloride to do away with an absorptive medium. To prevent reflected light, the back of the plate was covered with asphaltum varnish, and frequently a small metal disc a little larger than the sun's image was interposed in front of the plate to cut off the sun's direct light.

The next consideration was as to the optical means by which an image of the sun, as free as possible from imperfections of any kind, could be formed upon the plate. For several obvious reasons the use of lenses was given up, and I turned to reflection from a mirror of speculum metal. My first experiments were made with a Newtonian telescope by Short. With this instrument, during the summer of 1882 about twenty plates were taken on different days, in all of which coronal forms are to be seen about the sun's image. After a very critical examination of these plates, in which I was greatly helped by the kind assistance of Professor Stokes and Captain Abney, there seemed to be good ground to hope that the corona had really been obtained on the plates. [One of these negatives, obtained in August 1882, was shown upon the screen.]

In the spring of the following year, 1883, the attack upon the corona was carried on with a more suitable apparatus. The Misses Lassell were kind enough to lend me a seven-foot Newtonian telescope made by Mr. Lassell which possesses great perfection of figure and retains still its fine polish. For the purpose of avoiding the disadvantage of a second reflection from the small mirror, and also of reducing the aperture to 3½ inches, which gives a more manageable amount of light, I adopted the arrangement of the instrument which is shown in the following woodcut.



The speculum *b* remains in its place at the end of the tube *a*, *a*, by which the mechanical inconvenience of tilting the speculum within in the tube as in the ordinary form of the Herschelian telescope is avoided.

The small plane speculum and the arm carrying it were removed. The open end of the tube is fitted with a mahogany cover. In this cover at one side is a circular hole *f*, 3½ inches diameter, for the light to enter; below is a similar hole over which is fitted a framework to receive the "backs" containing the photographic plates, and also to receive a frame with fine-ground glass for putting the apparatus into position. Immediately below, towards the speculum, is fixed a shutter with an opening of adjustable width, which can be made to pass across more or less rapidly by the use of indiarubber bands of different degrees of strength. In front of the opening *f* is fixed a tube *c*, six feet long, fitted with diaphragms, to restrict as far as possible the light which enters the telescope to that which comes from the sun and the sky immediately around it. The telescope-tube *a*, *a*, is also fitted with diaphragms, which are not shown in the diagram, to keep from the plate all light, except that coming directly from the speculum. It is obvious that, when the sun's light entering the tube at *f* falls upon the central part of the speculum, the image of the sun will be formed in the middle of the second opening at *d*, about two inches from the position it would take if the tube were directed axially to the sun. The exquisite definition of the photographic images of the sun shows, as was to be expected, that this small deviation from the axial direction, two inches in seven feet, does not affect sensibly the performance of the mirror. The whole apparatus is firmly strapped on to the refractor of the equatorial in my observatory, and carried with it by the clock motion.

The performance of the apparatus is very satisfactory. The photographs show the sun's image sharply defined; even small spots are seen. When the sky is free from clouds, but presents a whity appearance from the large amount of scattered light, the sun's image is well defined upon a uniform background of illuminated sky, without any sudden increase of illumination immediately about it. It is only when the sky becomes clear and blue in colour that coronal appearances present themselves with more or less distinctness. [Several negatives taken during the summer of 1883 were shown on the screen.] In our climate the increased illumination of the sky where there is a background of coronal light is too small to permit the photographs which show this difference to be otherwise than very faint. A small increase of exposure, or of development, causes it to be lost in the strong photographic action of the air-glare. For this reason, the negatives should be examined under carefully arranged illumination. They are not, therefore, well adapted for projection on a screen. [A negative taken with a whity sky, showed a well-defined image of the sun, with a sensibly uniform surrounding of air-glare, but without any indication of the corona. In the case of the other negatives exhibited, which were taken on clearer days, an appearance, very coronal in character, was to be seen about the sun.]

On May 6, the corona was photographed during a total eclipse at Caroline Island by Messrs. Lawrence and Woods. This circumstance furnished a good opportunity of subjecting the new method to a crucial test, namely, by making it possible to compare the photographs taken in England where there was no eclipse, with those taken at Caroline Island of the undoubtedly true corona during the eclipse. On the day of the eclipse the weather was bad in this country, but plates were taken before the eclipse, and others taken later on. These plates were placed in the hands of Mr. Wesley, who had had great experience in making drawings from the photographs taken during former eclipses. Mr. Wesley drew from the plates before he had any information of the results obtained at Caroline Island, and he was therefore wholly without bias in the drawings which he made from them. [Photographs of Mr. Wesley's drawings were projected on the screen, and then a copy of the Caroline Island eclipse photograph. The general resemblance was unmistakable, but the identity of the object photographed in England and at Caroline Island was placed beyond doubt by a remarkably formed rift on the east of the north pole of the sun. This rift, slightly modified in form, was to be seen in a plate taken about a solar rotation period before the eclipse, and also on a plate taken about the same time after the eclipse. The general permanence of this great rift certainly extended over some months, but no information is given as to whether the corona rotates with the sun. For from the times at which the plates were taken, one about a rotation period before and the other a rotation period after the eclipse, it is obvious the rift might have gone round with the sun, but there is no positive evidence on this point.*]

As the comparison of the English plates with those taken at Caroline Island possesses great interest, I think it well to put on record here a letter written by Mr. Lawrence to Professor Stokes, dated September 14, 1883:—

"Dr. Huggins called upon Mr. Woods this morning and showed us the drawings Mr. Wesley has made of his coronas. He told us that he particularly did not wish to see our negatives, but that he would like us to compare his results with ours. We did so, and found that some of the strongly marked details could be made out on his drawings, a rift near the north pole being especially noticeable; this was in a photograph taken on April 3, in which the detail of the northern hemisphere is best shown, while the detail of our southern hemisphere most resembles the photograph taken on June 6; in fact, our negatives seem to hold an intermediate position. Afterwards I went with Dr. Huggins and Mr. Woods to Burlington House to see the negatives. The outline and distribution of light in the inner corona of April 3 is very similar to that on our plate which had the shortest exposure; the outer corona is, however, I think, hidden by atmospheric glare. As a result of the comparison, I should say that Dr. Huggins's coronas are certainly genuine as far as 8′ from the limb."

Though the plates which were obtained during the summer of 1883 appeared to be satisfactory to the extent of showing that there could be little doubt remaining but that the corona had been photographed without an eclipse, and therefore of justifying the hope that a successful method for the continuous investigation of the corona had been placed in the hands of astronomers, yet as the photographs were taken under the specially unfavourable conditions of our climate, they failed to show the details of the structure of the corona.

---

* See Plates XI. and XIA, British Association Report, 1883, p. 318.

The next step was obviously to have the method carried out at some place of high elevation, where the large part of the glare which is due to the lower and denser parts of our atmosphere would no longer be present. I ventured to suggest to the Council of the Royal Society that a grant from the fund placed annually by the Government at the disposal of the Royal Society, should be put in the hands of a small committee for this purpose. This suggestion was well received, and a committee was appointed by the Council of the Royal Society. The committee selected the Riffel near Zermatt in Switzerland, a station which has an elevation of 8500 feet, and the further advantages of easy access, and of hotel accommodation. The committee was fortunate in securing the services, as photographer, of Mr. Ray Woods, who as assistant to Professor Schuster had photographed the corona during the eclipse of 1882 in Egypt, and who in 1883, in conjunction with Mr. Lawrence, had photographed the eclipse of that year at Caroline Island.

Mr. Woods arrived at the Riffel in the beginning of July 1884, with an apparatus, similar to one shown in the woodcut on a former page, constructed by Mr. Grubb.

Captain Abney who had made observations on the Riffel in former years, had remarked on the splendid blue-black skies which were seen there whenever the lower air was free from clouds or fog. But unfortunately during the last year or so a veil of finely divided matter of some sort has been put about the earth, of which we have heard so much in the accounts from all parts of the earth of gorgeous sunsets and afterglows. This fine matter was so persistently present in the higher regions of the atmosphere during last summer, that Mr. Woods did not get once a really clear sky. On the contrary, whenever visible cloud was absent, then instead of a blue-black sky there came into view a luminous haze, forming a great aureole about the sun, of a faint red colour, which passed into bluish white near the sun. Mr. Woods found the diameter of the aureole to measure about 44°. This appearance about the sun has been seen all over the world during last summer, but with greatest distinctness at places of high elevation.

The relative position of the colours, blue inside and red outside, shows that the aureole is a diffraction phenomenon due to minute particles of matter of some kind. Mr. Ellery, Captain Abney, and some others consider the matter to be water in the form probably of minute ice spicules; others consider it to consist of particles of volcanic dust projected into the air during the eruption at Krakatoa; but whatever it is, and whencesoever it came, it is most certainly matter in the wrong place so far as astronomical observations are concerned, and in a peculiar degree for success in photographing the corona. We are only beginning to learn that whether in our persons or in our works, it is by minimised matter chiefly that we are undone. So injurious was the effect of this aureole that it was not possible to obtain any photographs of the corona at my observatory near London. This great diffraction aureole went far to defeat the object for which Mr. Woods had gone to the Riffel, but fortunately the great advantage of being free from the effects of the lower 8000 feet of denser air told so strongly, that notwithstanding the ever-present aureole Mr. Woods was able to obtain a number of plates on which the corona shows itself with more or less distinctness. [Three untouched photographic copies of the plates taken at the Riffel were shown upon the screen.] From the presence of the aureole the negatives show less detail than we have every reason to believe would have been the case if the sky had been as blue and clear as in some former years. This circumstance makes great care necessary in the discussion of these plates, and it would be premature to say what information is to be obtained from them.

[As an illustration of the differences of form which the corona has assumed at different eclipses, photographs taken in 1871, 1878, 1882, and 1883 were projected on the screen. Attention was called to the equatorial extension seen in the photograph taken in 1878, and to the suggestion which had been put forward that this peculiar character was connected with the then comparative state of inactivity of the sun's surface, at a period of minimum sun-spot action, especially as an equatorial extension was observed in 1867.]

It is now time that something should be said of the probable nature of the corona.

Six hypotheses have been suggested:—

1. That the corona consists of a gaseous atmosphere resting upon the sun's surface and carried round with it.

2. That the corona is made up, wholly or in part, of gaseous and finely divided matter which has been ejected from the sun, and is in motion about the sun from the forces of ejection, of the sun's rotation, and of gravity,—and possibly of a repulsion of some kind.

3. That the corona resembles the rings of Saturn, and consists of swarms of meteoric particles revolving with sufficient velocity to prevent their falling into the sun.

4. That the corona is the appearance presented to us by the unceasing falling into the sun of meteoric matter and the débris of comets' tails.

5. That the coronal rays and streamers are, at least in part, meteoric streams strongly illuminated by their near approach to the sun, neither revolving about nor falling into the sun, but permanent in position and varying only in richness of meteoric matter, which are parts of eccentric comet orbits. This view has been supported by

Mr. Proctor, on the ground that there must be such streams crowding richly together in the sun's neighbourhood.

6. The view of the corona suggested by Sir William Siemens in his solar theory.

It has been suggested, even, that the corona is so complex a phenomenon that there may be an element of truth in every one of these hypotheses. Any way this enumeration of hypotheses more or less mutually destructive, shows how great is the difficulty of explaining the appearances which present themselves at a total solar eclipse, and how little we really know about the corona.

An American philosopher, Professor Hastings, has revived a prior and altogether revolutionary question: Has the corona an objective existence? Is it anything more than an optical appearance depending upon diffraction? Professor Hastings has based his revival of this long discarded negative theory upon the behaviour of a coronal line which he saw, in his spectroscope, change in length east and west of the sun during the progress of the eclipse at Caroline Island. His view appears to rest on the negative foundation that Fresnel's theory of diffraction may not apply in the case of a total eclipse, and that at such great distances there is a possibility that the interior of the shadow might not be entirely dark, and so to an observer might cause the appearance of a bright fringe around the moon.*

Not to speak of the recent evidence of the reality of the corona from the photographs which have been taken when there is no intervening moon to produce diffraction, there is the adverse evidence afforded by the peculiar spectra of different parts of the corona and by the complicated and distinctly peculiar structure seen in the photographs taken at eclipses. The crucial test of this theory appears to be, that if it be true, then the corona would be much wider on the side where the sun's limb is least deeply covered, that is to say the corona would alter in width on the two sides during the progress of the eclipse. Not to refer to former eclipses where photographs taken at different times and even at different places have been found to agree, the photographs taken during the eclipse at Caroline Island show no such changes. M. Janssen says: "Les formes de la couronne ont été absolument fixes pendant toute la durée de la totalité." The photographs taken by Messrs. Lawrence and Woods also go to show that the corona suffered no such alterations in width or form as would be required by Professor Hastings' theory during the passage of the moon.

We have therefore, I venture to think, a right to believe in an objective reality of some sort about the sun corresponding to the appearance which the corona presents to us. At the same time some very small part of what we see must be due to a scattering of the coronal light itself by our air, but the amount of this scattered light over the corona must be less than what is seen over the dark moon.

---

* Report of the Eclipse Expedition to Caroline Island, May 1883. Memoir of the National Academy of Sciences, Washington.

That the sun is surrounded by a true gaseous atmosphere of relatively limited extent there can be little doubt, but many considerations forbid us to think of an atmosphere which rises to a height which can afford any explanation of the corona, which streams several hundred thousand miles above the photosphere. For example, a gas at that height, if hundreds or even thousands of times lighter than hydrogen, would have more than metallic density near the sun's surface, a state of things which spectroscopic and other observations show is not the case. The corona does not exhibit the rapid condensation towards the sun's limb which such an atmosphere would present, especially when we take into account the effect of perspective in increasing the apparent brightness of the lower regions of the corona. There is, too, the circumstance that comets have passed through the upper part of the corona without being burnt up or even sensibly losing velocity.

There can scarcely be doubt that matter is present about the sun wherever the corona extends, and further that this matter is in the form of a fog. But there are fogs and fogs. The air we breathe, when apparently pure, stands revealed as a dense swarming of millions of motes if a sunbeam passes through it. Even such a fog is out of the question. If we conceive of a fog so attenuated that there is only one minute liquid or solid particle in every cubic mile, we should still have matter enough, in all probability, to form a corona. That the coronal matter is of the nature of a fog is shown by the three kinds of light which the corona sends to us. Reflected solar light scattered by particles of matter solid or liquid, and secondly light giving a continuous spectrum, which tells us that these solid or liquid particles are incandescent, while the third form of spectrum of bright lines, fainter and varying greatly at different parts of the corona and at different eclipses, shows the presence also of light-emitting gas. This gas existing between the particles need not necessarily form a true solar atmosphere, which the considerations already mentioned make an almost impossible supposition, for we may well regard this thin gas as carried up with the particles, or even to some extent to be furnished by them under the sun's heat.

It will be better to consider first the probable origin of this coronal matter, and by what means it can find itself at such enormous heights above the sun.

There is another celestial phenomenon, very unlike the corona at first sight, which may furnish us possibly with some clue to its true nature. The head of a large comet presents us with luminous streamers and rifts and curved rays, which are not so very unlike, on a small scale, some of the appearances which are peculiarly characteristic of the corona.* We do not know for certain the conditions under which these cometary appearances take place, but the hypothesis which seems on the way to become generally accepted, attributes them to electrical disturbances, and especially to a repulsive force acting from the sun, possibly electrical, which varies as the surface and not like gravity as the mass. A force of this nature in the case of highly attenuated matter can easily master the force of gravity, and as we see in the tails of comets, blow away this thin kind of matter to enormous distances in the very teeth of gravity.

---

* See "Comets," Royal Institution Proceedings, vol. x. p. 1.

If such a force of repulsion is experienced in comets, it may well be that it is also present in the sun's surroundings. If this force be electrical it can only come into play when the sun and the matter subjected to it have electric potentials of the same kind, otherwise the attraction on one side of a particle would equal the repulsion on the other. On this theory, the coronal matter and the sun's surface must both be in the same electrical state, the repelled matter negative if the sun is negative, positive if the sun is positive.

The grandest terrestrial displays of electrical disturbance, as seen in lightning and the aurora, must be of a small order of magnitude as compared with the electrical changes taking place in connection with the ceaseless and fearful activity of the sun's surface, but we do not know how far these actions, or the majority of them, may be in the same electrical direction, or what other conditions there may be, so as to cause the sun's surface to maintain a high electrical state, whether positive or negative. A permanence of electric potential of the same kind would seem to be required by the phenomena of comets' tails.

If such a state of high electric potential at the photosphere be granted as is required to give rise to the repulsive force which the phenomena of comets appear to indicate, then considering the gaseous irruptions and fiery storms of more than Titanic proportions which are going on without ceasing at the solar surface, it does not go beyond what might well be, to suppose that portions of matter ejected to great heights above the photosphere and often with velocities not far removed from that which would be necessary to set it free from the sun's attraction, and very probably in the same electric state as the photosphere, might so come under this assumed electric repulsion as to be blown upwards and to take on forms such as those seen in the corona: the greatest distances to which the coronal streamers have been traced are small as compared with the extent of the tails of comets, but then the force of gravity which the electrical repulsion would have to overcome near the sun would be enormously greater.

It is in harmony with this view of things that the positions of greatest coronal extension usually correspond with the spot zones where the solar activity is most fervent; and also that a careful examination of the structure of the corona suggests strongly that the forces to which this complex and varying structure is due have their seat in the sun. Matter repelled upwards would rise with the smaller rotational velocity of the photosphere, and lagging behind would give rise to curved forms; besides, the forces of irruption and subsequent electrical repulsion might well vary in direction and not be always strictly radial, and under such circumstances a structure of the character which the corona presents might well result. The sub-permanency of any great characteristic coronal forms, as, for example, the great rift seen in the photographs of the Caroline Island eclipse and also in those taken in England a month before the eclipse and about a month afterwards, must probably be explained by the maintenance for some time of the conditions upon which the forms depend, and not to an unaltered identity of the coronal matter; the permanency belonging to the form only, and not to the matter, as in the case of a cloud over a mountain top, or of a flame over the mouth of a volcano. If the forces to which the corona is due have their seat in the sun, the corona would probably rotate with it; but if the corona is produced by conditions external to the sun, then the corona might not be carried round with the sun.

We have seen that the corona consists probably of a sort of incandescent fog, which at the same time scatters to us the photospheric light. Now we must bear in mind the very different behaviour of a gas, and of liquid or solid particles in the near neighbourhood of the sun. A gas need not be greatly heated, even when near the sun, by the radiated energy; heated gas from the photosphere would rapidly lose heat; but on the other hand liquid or solid particles, whether originally carried up as such, or subsequently formed by condensation, would absorb the sun's heat, and at coronal distances would soon rise to a temperature not very greatly inferior to that of the photosphere. The gas which the spectroscope shows to exist along with the incandescent particles of the coronal stuff, may therefore have been carried up as gas, or have been in part distilled from the coronal particles under the enormous radiation to which they are exposed. Such a view would not be out of harmony with the very different heights to which different bright lines may be traced at different parts of the corona and at different eclipses. For obvious reasons, gases of different vapour density would be differently acted upon by a repulsive force which varies as the surface and would to some extent be winnowed from each other; the lighter the gas the more completely would it come under the sway of repulsion, and so would be carried to a greater height than the gas more strongly held down by gravity. The relative proportions, at different heights of the corona, of the gases which the spectroscope shows to exist there (and recently Captain Abney and Professor Schuster have shown that in addition to the bright lines already known, the spectrum of the corona of 1882 gave the rhythmical group of the ultra-violet lines of hydrogen which are characteristic of the photographic spectra of the white stars, and some other lines also) would vary from time to time, and depend in part upon the varying state of activity of the photosphere, and so probably establish a connection with the spectra of the prominences. This view of the corona would bring it within the charmed circle of interaction which seems to obtain among the phenomena of sun-spots and terrestrial magnetic disturbances and auroræ.

Many questions remain unconsidered; among others, whether the light emitted by the gaseous part of the corona is due directly to the sun's heat, or to electrical discharges taking place in it of the nature of the aurora. Further, what becomes of the coronal matter on the theory which has been suggested? Is it permanently carried away from the sun, as the matter of the tails of comets is lost to them? Among other considerations it may be mentioned that electric repulsion can maintain its sway only so long as the repelled particle remains in the same electrical state: if through electric discharges it ceases to maintain the electrical potential it possessed, the repulsion has no more power over it, and gravity will be no longer mastered. If, when this takes place, the particle is not moving away with a velocity sufficiently great to carry it from the sun, the particle will return to the sun. Of course, if the effect of any electric discharges or other conditions has been to change the potential of the particle from positive to negative, or the reverse, as the case may be, then the repulsion would be changed into an attraction acting in the same direction as gravity. In Mr. Wesley's drawings of the corona, especially in those of the eclipse of 1871, the longer rays or streamers appear not to end, but to be lost in increasing faintness and diffusion, but certain of the shorter rays are seen to turn round and to descend to the sun.*

It is difficult for us living in dense air to conceive of the state of attenuation probably present in the outer parts of the corona. Mr. Johnstone Stoney has calculated that more than twenty figures are needed to express the number of molecules in a cubic centimetre of ordinary air, and Mr. Crookes shows us in his tubes that matter, even when reduced to one-millionth part of the density of ordinary air, can become luminous under electrical excitement. [A glass bulb about 4 inches in diameter, kindly lent to me by Mr. Crookes, was exhibited, in which a metal ball about half an inch in diameter formed the negative pole. Under a suitable condition of the induction current, this ball was seen to be surrounded by a corona of blueish-grey light which was sufficiently bright to be seen from all parts of the theatre.] Yet it is probable that these tubes must be looked upon as crowded cities of molecules as compared with the sparse molecular population of the great coronal wastes.

I forbear to speculate further, as we may expect more information as to the state of things in the corona from the daily photographs which will be shortly commenced at the Cape of Good Hope by Mr. Ray Woods under the direction of Dr. Gill. [W. H.]

---

* For a history of opinion of the nature of the corona, see Papers by Prof. Norton, Prof. Young, and Prof. Langley in the 'American Journal of Science'; also 'The Sun,' by Prof. Young; and 'The Sun the Ruler of the Planetary System,' and various essays by Mr. R. A. Proctor.

WEEKLY EVENING MEETING,

Friday, February 27, 1885.

HENRY POLLOCK, Esq. in the Chair.

PROFESSOR RAY LANKESTER, M.A. LL.D. F.R.S.

A Marine Biological Laboratory.

THE lecturer stated that he had been invited, as Secretary of the Marine Biological Association, of which Professor Huxley was President, H.R.H. the Prince of Wales, Patron, and all our leading naturalists members, to explain the objects of the Association. The immediate purpose of the Association was to erect a laboratory on the sea-coast where naturalists might resort for the purpose of studying, with the aid of the best possible apparatus and other facilities, the structure and life-history of marine plants and animals.

This study was not only important as a rapidly growing branch of science, but had value for the practical man, in that the proper management of our sea-fisheries must depend on the knowledge gained in such laboratories as that about to be erected.

The lecturer then gave some account of an investigation made by him into the causes of the colour of the "green beard" oysters, or *huîtres de Marenne*, as an example of the kind of work which would be carried on in a marine laboratory, and of the apparatus needed for such work.

He then mentioned what had been done by other countries in the way of providing laboratories on the sea-coast. The governments of France and the United States were especially signalised as having recognised the commercial and national importance of a thoroughly scientific study of sea-fishes and shell-fish, the latter giving now a yearly grant of seventy thousand pounds to Professor Baird for the purpose of laboratories and experiments in fish-culture. The admirable marine laboratory organised at Naples by Dr. Dohrn, and the smaller institutions at Trieste, at Newport and Beaufort, U S.A. were described. Drawings of the Naples laboratory and of that at Wood's Holl, U.S.A. were exhibited.

A map of Plymouth Sound was then referred to, and the advantages of Plymouth as a site for a marine laboratory were explained—being these, viz. the occurrence of a varied and abundant fauna in the waters of the Sound and the presence of a large fleet of fishing boats. The Marine Biological Association had obtained permission to erect a laboratory upon an admirable site on the Citadel view of Plymouth—a sketch of which was exhibited. Five thousand pounds had been raised by the Association by subscriptions from scientific men and their friends, and from important bodies interested in the enterprise as one of a national character tending to develop commercial and industrial resources—such as the Corporation of the City of London, the Clothworkers', Mercers', Skinners' and Goldsmiths' Companies. The Association required another five thousand pounds before commencing to build its laboratory—and this the lecturer hoped to see soon provided.

[R. L.]

GENERAL MONTHLY MEETING,

Monday, March 2, 1885.

Sir Frederick Pollock, Bart. M.A. Manager and Vice-President, in the Chair.

A. Noel Agnew, Esq.
Alfred Barnard Basset, Esq. M.A.
Richard H. Beauchamp, Esq.
Charles Harrison, Esq.
James Hole, Esq.
Thomas John Maclagan, M.D.
William Haynes Smith, Esq.
Richard Everard Webster, Esq. Q.C.

were elected Members of the Royal Institution.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Palæontologia Indica: Series IV. Vol. I. Part 4. 4to. 1885.

The New Zealand Government—Statistics of the Colony of New Zealand, 1883. fol. 1884.

The Corporation of the City of London—London's Roll of Fame, 1757-1884. 4to. 1884.

Accademia dei Lincei, Reale, Roma—Atti Serie Quarta: Rendiconti. Vol. I. Fasc. 1-4. 8vo. 1884-5.

Asiatic Society of Bengal—Proceedings, No. 10. 8vo. 1884.
Journal, Vol. LII. Part II. 8vo. 1883.

Astronomical Society, Royal—Monthly Notices, Vol. XLV. No. 3. 8vo. 1885.

Bagot, Alan, Esq. (the Author)—Principles of Civil Engineering as applied to Agriculture and Estate Management. 12mo. 1885.

Bankers, Institute of—Journal, Vol. V. Part 9; Vol. VI. Parts 1 and 2. 8vo. 1884-5.

Boston Society of Natural History—Memoirs, Vol. III. Nos. 8-10. 4to. 1884.
Proceedings, Vol. XXII. Parts 2, 3. 8vo. 1883-4.

British Architects, Royal Institute of—Proceedings, 1884–5, Nos. 8, 9. 4to.

Brussels, University of—Notice Historique. Par L. Vanderkindere [1831–1884]. 8vo. 1884.

Cambridge Philosophical Society—Proceedings, Vols. I. and II. 1843–1876. 8vo.

Chemical Society—Journal for February, 1885. 8vo.
Index to Journal, Vols. XLV. and XLVI.

Dilettanti Society—An Account of the Portraits. 8vo. 1885.

East India Association—Journal, Vol. XVII. No. 2. 8vo. 1885.

Editors—American Journal of Science for February, 1885. 8vo.
Analyst for February, 1885. 8vo.
Athenæum for February, 1885. 4to.
Chemical News for February, 1885. 4to.
Engineer for February, 1885. fol.
Horological Journal for February, 1885. 8vo.
Iron for February, 1885. 4to.
Nature for February, 1885. 4to.
Revue Scientifique and Revue Politique et Littéraire for February, 1885. 4to.
Science Monthly, Illustrated, for February, 1885.
Telegraphic Journal for February, 1885. 8vo.

Franklin Institute—Journal, No. 710. 8vo. 1885.

Geographical Society, Royal—Proceedings, New Series, Vol. VII. No. 2. 8vo. 1885.

Geological Society—Quarterly Journal, No. 161. 8vo. 1885.

Johns Hopkins University—University Circular, No. 36. 4to. 1884.

Linnean Society—Transactions: Botany, Vol. II. Part 8; Zoology, Vol. II. Parts 11, 13, 14; Vol. III. Part 2. 4to. 1884.

Meteorological Office—Monthly Weather Report, November, 1884. 4to.
Principles of Forecasting. By R. Abercromby. 8vo. 1885.

National Association for Social Science—Proceedings, Vol. XVII. No. 4. 8vo. 1884.

North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXIV. Part 1. 8vo. 1885.

Odontological Society of Great Britain—Transactions, Vol. XVII. No. 4. New Series. 8vo. 1885.
Supplementary Catalogue of the Museum. 8vo. 1884.

Pharmaceutical Society of Great Britain—Journal, February, 1885. 8vo.

Photographic Society—Journal, New Series, Vol. IX. No. 4. 8vo. 1885.

Seismological Society of Japan—Transactions, Vol. VII. Part 2. 8vo. 1884.

Smith, Willoughby, Esq. M.R.I. (the Author)—Induction and Conduction. 8vo. 1885.

Society of Arts—Journal, February, 1885. 8vo.

St. Bartholomew's Hospital—Reports, Vol. XX. 8vo. 1884.

St. Petersbourg, Académie des Sciences—Bulletins, Tome XXIX. No. 4. 4to. 1884.

United Service Institution, Royal—Journal, No. 127. 8vo. 1885.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1885: Heft 1. 4to.

WEEKLY EVENING MEETING,

Friday, March 6, 1885.

Joseph Brown, Esq. Q.C. Manager, in the Chair.

Charles T. Newton, C.B. LL.D. M.A.

The German Discoveries at Pergamus.

(No Abstract.)

WEEKLY EVENING MEETING,

Friday, March 13, 1885.

Sir Frederick Bramwell, F.R.S. Manager and Vice-President, in the Chair.

Sir Frederick Abel, C.B. D.C.L. F.R.S. *M.R.I.*

Accidental Explosions produced by Non-explosive Liquids.

Ten years ago the lecturer discussed in some detail the various causes of the continually recurring casualties which are classed under the head of accidental explosions, and he then had occasion to compare the causes of coal-gas explosions, the occurrence of which is as deplorably frequent now as it was then, with those of accidents connected with the transport, storage, and use of volatile inflammable liquids which are receiving extensive application, chiefly as solvents and as illuminating agents.

Within the last few years he has had occasion to devote special attention to the investigation of instances of this class of accident, and to examine more particularly into the probable causes of frequent casualties connected with the employment of lamps in which the various products included under the general designations of petroleum and paraffin oil are burned. The latter branch of these inquiries, which is still in progress, has been conducted in association with Mr. Boverton Redwood, the talented Secretary and Chemist of the Petroleum Association, and with the valuable aid of Dr. W. Kellner, Assistant-Chemist of the War Department. Although it may be hoped that their continuation will lead to further data and conclusions of practical and public importance, it is thought that some account of facts already elicited may interest the members of the Royal Institution, and possess some general value.

Ever since liquids which, more or less rapidly, evolve inflammable vapour when freely exposed to air, or partially confined, have been in extensive use, casualties have occurred from time to time through the accidental or thoughtless ignition of the mixtures of vapour and air thus formed, whereby more or less violent and destructive explosions have been produced, often followed by the ignition of the exposed liquid which is the source of the explosive mixture, and by the consequent frequent development of disastrous conflagrations.

Many instances are on record of explosions, sufficiently violent to produce effects destructive or injurious to life and property, resulting from the application of flame to vessels which had contained either the more volatile coal-tar or petroleum products, or strong spirituous liquids, and which, though they had been entirely or nearly emptied of their contents, still contained, or retained by absorption within their body, some of the volatile liquid, this having, by evaporation into the air in the emptied receptacle, produced with it a more or less violently explosive mixture. Thus, a loud explosion occurred at the entrance of a lamp-maker's shop in Whitecross Street, which was found to have been caused by a boy throwing a piece of lighted paper into a cask standing under the gateway, which had contained benzoline; two boys were very seriously injured by the blast of flame which was projected from the barrel. A perfectly analogous accident was soon afterwards reported in the papers as having occurred at Sheffield, with serious injury to the author of the catastrophe and another boy; and a very similar case occurred at Exeter during the removal of some empty benzoline barrels, consequent upon a boy applying a lighted match to the hole of one of them. Again, at Spaxton in Somersetshire, a young man applied a light to the hole of a benzoline cask described as nearly empty which was standing in the road, when three young men were blown across the road, one of them being so seriously injured about the head that he died.

Explosions with similarly disastrous results have also been publicly recorded as having resulted from the application of a light to rum puncheons and whisky casks, even some time after they have been emptied of their contents, the evaporation of the alcohol absorbed by the wood having sufficed to convert the confined air into a violently explosive mixture.

The readiness or extent to which inflammable vapour is evolved from those products of the distillation of petroleum, or of shale or coal, which are used for illuminating purposes, differs of course considerably with the character of these liquids. Those which are classed as petroleum spirit (known as gasoline, benzine, benzoline, naphtha, japanners' spirit, &c.), and in regard to which there exist very special precautionary enactments, are, it need scarcely be said, of far more dangerous character than those classed as burning oils, which include the paraffin oils obtained from shale and the so-called flashing points of which range from 73° to above 140° Fahrenheit. The rapidity with which the vapours, evolved by the more volatile products on exposure to air, or by their leakage from casks or barrels, diffuse themselves through the air, producing with it more or less violent explosive mixtures, has been a fruitful source of disaster, sometimes of great magnitude. The lecturer had occasion to refer, in his discourse of 1875, to an accident at the Royal College of Chemistry of which he was a witness, in 1847, when the lamented Mr. C B. Mansfield was engaged in the conversion of a quantity of benzol into nitrobenzol in a capacious glass vessel, which suddenly cracked, allowing the warm liquid hydrocarbon to escape and flow over a large surface. This occurred in an apartment 38 feet long, about 30 feet wide, and 10 feet high; there was a gas jet burning at the extremity of the room opposite to that where the heated liquid was spilled, and within a very brief space of time after the vessel broke, a sheet of flame flashed from the gas jet along the upper part of the room, to the spot where the fluid lay scattered.

The origin of a fire which occurred at some mineral oil stores at Exeter in 1882, affords another striking illustration of the great rapidity with which the vapour of petroleum spirit will diffuse itself through the air. The store which caught fire, and which contained both petroleum oil and spirit, or benzoline, was one of a range of arched caves upon the bank of a canal, being separated from it by a roadway about 50 feet wide. It was a standing rule at the stores that no light should be taken to any one containing benzoline. The benzoline casks were to be removed from this store, and the foreman, desirous of beginning the work early, and forgetful of the rule, went to the store shortly before daylight, carrying a lighted lantern, which he placed upon the ground at a distance of several feet from the doors. He then proceeded to open these. As he did so, he noticed a very powerful odour of benzoline and, almost immediately, he saw a flash of flame proceed from the lantern to the store. He had just turned to escape, when an explosion occurred which blew the doors and the lantern across the canal; the benzoline in the store was at once inflamed, and flowed out into the road and upon the surface of the water, firing a small vessel which lay against the quay, and setting fire to the stores of benzoline contained in two neighbouring caves.

Many exemplifications might be cited of the danger arising from the accidental spilling or escape of petroleum spirit (or even of oils of very low flashing point) in the ordinary course of dealing with these liquids, as in stores where there is but very imperfect ventilation, and in some part of which a flame exists, or is carelessly introduced; or, from the escape of spirit or its vapour from stores or receptacles to adjacent spaces where, its existence being unsuspected, the ignition of the resulting explosive mixture of vapour and air may be at any time brought about.

Without referring to accidents which have been due to flagrant carelessness in introducing a flame or striking a light in a store where petroleum vapour is likely to exist in the air, or where some form of spirit has been accidentally spilled, a few instances may be quoted which illustrate the magnitude of casualties liable to arise from the causes just referred to. Some years ago an explosion productive of much damage occurred in a sewer at Greenwich, and was clearly traced to the entrance into the sewer of some petroleum products (from a neighbouring patent gas factory); the vapours from these had diffused themselves through the air in the sewer to a considerable distance, forming with it an explosive mixture which must have been accidentally ignited at one of the sewer openings in the street above. Last spring a similar accident occurred at Newport in Monmouthshire, a quantity of benzoline having escaped into a sewer from a neighbouring store; the ignition of the resulting explosive mixture of vapour and air, with which a considerable length of the sewer became filled, tore up the roadway to some distance, several persons being thrown down. A terrific disaster of the same class was reported from San Francisco in November 1879. During the driving of a tunnel in the San José Santa Cruz Railway, a vein of petroleum became exposed by the excavators, who were of course working with naked lights. Three violent explosions occurred in consequence, in rapid succession, resulting in the death of twenty-five Chinamen and in the injury of seventeen others and two white men.

Another accident, which occurred near Coventry nearly five years ago, may be quoted in illustration of the unsuspected manner in which explosive gas-mixtures may exist in localities which, to the superficial observer, may appear to have no connection with a neighbouring locality where volatile liquids are liable to escape confinement.

A dealer in benzoline spirit kept his small store of that liquid (from 20 to 80 gallons) in an apartment of his house, upon the basement, the floor of the room being paved with red bricks. At a distance of about three feet from the store room there was a well, the depth of which to the surface of the water was 20 feet. The well was closed in almost entirely with planks covered with earth. The water in the well being found foul, the owner had the latter uncovered, with a view to its being cleared out. The workman in charge of the operation, after having been engaged for three hours in pumping out a large quantity of the water, lowered a lighted candle into the well according to the usual practice to see whether he could descend with safety, when, while bending over the opening, he perceived a blue flame shooting upwards, and was violently thrown back and badly burnt, a woman who was watching him being similarly injured. The benzoline which had been spilled from time to time in small quantities in filling the cans of customers had readily passed through the porous brick upon which it fell, and gradually permeating the soil beneath had, in course of time, drained into the adjacent well. That this must occur under the circumstances described would have been self-evident to any one acquainted with the behaviour of these liquids and with the attendant circumstances. In localities where large quantities have for some time been stored in the usual casks or barrels, there is no difficulty in "striking oil" by sinking a well in the immediately adjacent ground, in consequence of the large amount of leakage of the spirit or oil which must unavoidably occur. Even in the absence of leakage from the openings of the barrels or from any accidental imperfection, considerable diffusion of the volatile liquid and consequent escape by evaporation through the wood itself, must occur in large petroleum stores especially if much exposed to the sun, and in the holds of ships where the temperature is generally more or less high. Even the precaution adopted of rinsing the barrels before use with a stiff solution of glue is not effectual in preventing the escape of the spirit from these causes, as the effect of alternations of temperature upon the barrels must tend to re-open any unsound places temporarily closed by the glue. Even at very extensive depots, where special arrangements were adopted to maintain the stores uniformly at a very moderate average temperature, the loss of petroleum spirit from leakage and evaporation was estimated, ten years ago, to amount to about 18 per cent. of the total stored, while the average loss from the same causes upon petroleum oil was about 9 per cent. By the introduction from time to time of improvements of the arrangements, the loss of spirit by leakage and evaporation has been very considerably reduced, amounting to less than 8 per cent. in well-constructed stores, while at some petroleum stores, more especially in Germany, the loss of oil from leakage is now said not to exceed 1 per cent.

As in the case of the loss of coal-laden ships by explosions on the high seas, such loss has probably in many cases been due to the development of gas from the cargo, and to its diffusion into the air of parts of the ship more or less distant from the coal, producing an explosive atmosphere which might become ignited by the conveyance or existence of a light or fire, where its presence was not deemed dangerous; so also it is not improbable that the supposed loss by effects of weather, of missing petroleum-laden vessels, may have occasionally arisen from fire caused in the first instance by the diffusion of vapour, escaping from the cargo, through the air in contiguous parts of the ship, and the accidental ignition of the explosive atmosphere thus produced.

The possibility of such disasters has been demonstrated by the repeated occurrence of accidents of this class in ports or their vicinity. A very alarming instance of the kind occurred in 1871 on the Thames at Erith. Two brigantines had nearly completed the discharge of their cargoes of petroleum spirit ("naphtha"), when another vessel, the *Ruth*, from Nova Scotia, containing upwards of 2000 barrels of the same material, together with other inflammable cargo, anchored alongside them. This ship had encountered very severe weather, and it had been necessary to batten down the hatches; the cargo in the hold had consequently become enveloped in the vapour which had escaped from the casks. On the removal of the hatches, an explosive mixture was speedily produced by access of air, and, through some unexplained cause, became ignited shortly after the vessel anchored. A violent explosion followed, and the vessel was almost instantly in flames, the fire being rapidly communicated to the other two ships, which were with difficulty saved after sustaining considerable injury, while the *Ruth*, in which the fire raged uncontrollably, was after a time towed to a spot where she could burn herself out and sink, without damage to the other shipping. Three of the crew were seriously injured by the explosion, and the mate was blown to some distance into the water.

In June 1873 a vessel (the *Maria Lee*), laden with 300 barrels of petroleum and other inflammable cargo, was destroyed by fire on the Thames near the Purfleet powder magazines, consequent upon the explosion in her of a mixture of petroleum vapour and air, and a similar accident occurred about the same time in Glasgow harbour. In the case of the *Maria Lee* it was clearly proved that the vapour resulting from leakage and evaporation of the spirit in the hold had diffused itself through the ship during the night, which was very hot, the hatches having been kept closed and covered with tarpaulin, in consequence of the occurrence of a thunderstorm. Upon the captain entering his cabin in the after-part of the ship early in the morning (and probably striking a light) a loud explosion took place, and flame was immediately seen issuing from the fore-part of the ship.

A very similar casualty to the foregoing occurred at Liverpool four years afterwards, in a small vessel laden with petroleum spirit, which proved not to have been at all adapted by internal construction for the safe carriage of such a freight. The cargo of 214 barrels of spirit had been stowed on board, and the hatches were put down and covered with tarpaulin. The cabin and forecastle of the smack were below deck, and were only separated by a thin partition from the hold. The loading had been completed between 6 and 7 o'clock in the evening, and at about 8 o'clock the captain went into the cabin and kindled a lamp. A man upon deck, who with another was injured by the explosion and fire, saw the light burning in the forecastle, and almost immediately afterwards the deck was lifted and the man was thrown some distance, while flame issued from the hold. The captain was terribly burned, and died shortly afterwards. In vessels which are constructed for the American petroleum trade, the cabins and forecastles are all upon deck, that part of the vessel which carries the freight, between decks, being as completely as possible separated from the other parts of the ship.

In some instances, ships laden with petroleum oil have become inflamed, in an unexplained manner, without the occurrence of any noticeable explosion, as was the case last year with a large vessel (the *Aurora*) in the port of Calcutta, after she had discharged more than half her cargo of 59,000 cases. The vessel burned for nine hours, the river becoming covered with burning oil as she gradually filled with water; the direction of the wind and the condition of the tide at the time of her sinking fortunately prevented the fire from reaching the shipping higher up the river.

There is no doubt that, while with cargoes of the more volatile petroleum products, classed as spirit, the greatest precautions are necessary to guard against the possible ignition of more or less explosive mixtures of vapour and air which will be formed in the stowage spaces of ships, and which may extend to other parts of the vessels unless very efficient ventilation be maintained, ships laden with the oils produced for use in ordinary petroleum- or paraffin-lamps, and which, yielding vapours at temperatures above the standard fixed as a guarantee of safety, incur comparatively very little risk of accident, provided simple precautions be observed. If moreover, by some act of carelessness, or some accident not guarded against by the prescribed precautions, a part of such a cargo does become ignited, the prompt and, as far as practicable, complete exclusion of air from the seat of the fire, by the secure battening down of the hatches, will most probably save the ship from destruction. There are numerous records of vessels having discharged cargoes of petroleum oil, many barrels of which have been found greatly charred on the outside, occasionally even to such an extent that the receptacle has scarcely sufficient strength remaining to retain its contents. A remarkable illustration of the controllable nature of fire in a petroleum-laden ship was furnished by the ship *Joseph Fish*, laden with refined petroleum, lubricating oil, and turpentine, which, a fortnight after leaving New York (in September 1879), was struck by lightning during a heavy squall, the hatches being closed at the time. Smoke at once issued from below, and the force-pumps were set to work directly to keep the fire down. The hatches were removed for examination as the fire appeared to gain ground, but were immediately replaced, and, after further pumping, as the fire appeared to increase, and an explosion was feared, the crew took to their boats, remaining near the ship. Eight hours afterwards they were picked up by a passing ship, which remained near the *Joseph Fish* until daylight. Her captain then returned on board, and as he found that the fire appeared to be out, the crew returned and the ship resumed her voyage, reaching the port of London without further incident, except that during the use of the pumps for removing the water, considerable quantities of petroleum and turpentine were pumped up with it from the hold. When the cargo was discharged, a large number of the barrels bore evidence of the great heat to which they had been exposed; several casks had gone to pieces and the staves of others were charred quite half-way through, although they still retained their contents.

The lecturer had occasion, ten years ago, to dwell upon the recklessness with which fearful risks were incurred, in some cases no doubt ignorantly, but in others scarcely without a knowledge on the part of those who were responsible, of the nature of the materials dealt with, by transporting volatile and highly inflammable liquids together with explosive substances in barges or other craft, and in doing so, moreover, without the adoption of even the most obvious precautions for guarding against access of fire to the contents of those vessels. The instance of the explosion in 1864 of the *Lottie Sleigh* at Liverpool, laden with 11½ tons of gunpowder, in consequence of the accidental spilling and ignition of some paraffin oil in the cabin of the ship, illustrated the danger incurred in permitting these materials to be together on board a vessel, and should have furnished some warning by the publicity it received; but the explosion ten years later, on the Regent's Park canal, of the barge *Tilbury*, revealed the continued prevalence of the same reckless disregard of all dictates of common prudence in dealing with the joint transport of explosives and volatile inflammable liquids.

The efficient laws and Government inspection to which all traffic in explosives has since then been subject, has rendered the recurrence of that identical kind of catastrophe almost out of the question, but an illustration has not been wanting quite recently of the fact that, but for the respect commanded by the rigour of the law, barges passing through towns would probably still carry freights composed of petroleum spirit and powder or other explosives, being at the same time provided with a stove, lamp and matches for the convenient production of explosions. In August 1883 an explosion occurred on the canal at Bath, in a barge which sank immediately, the master being slightly injured; the freight of the vessel consisted of petroleum, benzoline, and lucifer-matches.

The last four years have furnished several very remarkable illustrations of great injuries inflicted on ships by explosions, the origin of which was traced to the existence on board of only small quantities of some preparation containing petroleum spirit, or benzoline, with the nature of which the men who had charge of them were not properly acquainted. These materials had consequently been so dealt with as to become the means of filling more or less confined spaces in the ships with an explosive atmosphere which, when some portion of it reached a flame, was fired throughout, with violently destructive effects.

The first authenticated case of an accident due to this cause occurred in June 1880, on board the Pacific Steam Navigation Company's steamer *Coquimbo* shortly after her arrival in the morning at Valparaiso from Coquimbo. A violent explosion took place, without any warning or apparent cause, in the fore-peak of the vessel, blowing out several plates of the bow and doing other structural damage, besides killing the ship's carpenter; the explosion could only be accounted for by the circumstance that a small quantity of a benzoline preparation used for painting purposes (probably as "driers") was stored in the fore-peak and that a mixture of the vapour from this with the air had become ignited. The sufferer was the only person who could have thrown light upon the precise cause of the accident, but there was no other material whatever in that part of the ship to which the explosion could have been in any way ascribed.

In May 1881 an explosion of a trifling character occurred on board H.M.S. *Cockatrice* in Sheerness Dockyard, in consequence of a man going into the store-room with a naked light and holding it close to a small can which was uncorked at the time, and which contained a preparation recently introduced into the naval service as a "driers" for use with paint, under the name of *Xerotine Siccative.* This preparation, which was of foreign origin, appears to have been adopted for use in the naval service and to have been issued to H.M. ships generally without any knowledge of its composition, and without attention being directed to the fact that it consisted very largely of the most volatile petroleum spirit, which would evaporate freely if the liquid were exposed to air at ordinary temperatures, and the escape of which from a can, jar or cask, placed in some confined and non-ventilated space, must speedily diffuse itself through the air, and render the latter more or less violently explosive.

When attention was directed to the highly inflammable character of this xerotine siccative by the slight accident referred to, official instructions were issued by the Admiralty, in June 1881, to ships and dockyards that the preparation should be stored and treated with the same precautions as turpentine and other highly inflammable liquids or preparations.

The following November, however, telegraphic news was received of a very serious explosion on board H.M.S. *Triumph*, then stationed at Coquimbo, due to the xerotine siccative. The explosion took place early in the evening of the 23rd November, and originated in one of the paint-rooms of the ship; the painter and a marine who was assisting him were in the upper paint-room at the time; the former received severe internal injuries and afterwards died, the latter was killed at once. One man standing at the open door of the sick bay furthest from the explosion was instantaneously killed, others in close proximity receiving only superficial injuries. Altogether there were two killed, two dangerously wounded—of whom one died, and six injured, by the explosion.

The results of the official inquiry held at Callao led to the conclusion that the explosion was caused by the ignition of an explosive gas-mixture produced by xerotine siccative which had leaked from a tin kept in a compartment under the paint-room and quite at the bottom of the ship, usually termed the "glory hole"; that locality having been considered by the captain of the ship as the safest place in which to keep this material, to the dangerous nature of which his attention had been recently called by the receipt of the Admiralty Circular. It transpired that the painter had sent his assistant down to this compartment from the paint-room to fetch some paint. The man, who had a hand-lantern with him, while unscrewing the hatch which had not been opened for three days, made the remark that there was a horrible smell; the chief painter told him to return, as he thought the smell was due to foul air, and immediately afterwards the explosion occurred.

The tin can which had contained six gallons of the liquid was found, after the accident, to have received injury as though some heavy body had fallen, or been placed, upon it; this appeared to have been done before the explosion, and there is no doubt that the liquid had leaked out of the can, and had evaporated into the air in the compartment beneath the paint-room, and probably also to some extent in the adjoining spaces. The damage done was very considerable. An iron ladder leading from one paint-room to the other was so twisted up as to have lost all semblance of originality, the wooden bulkhead separating the upper paint-room and sick bay was completely blown away, the framing of the ship's side in the sick bay was blown inwards and broken, the furniture in the latter was completely shattered, and the bedding and clothes of the men near the explosion were much burned. The inquiries which followed upon this deplorable accident showed that, while due precautions were taken to store the supplies of mineral oil used for burning purposes, of turpentine and of spirit, which were sent to different naval stations for supply to the fleet, in special parts of the ships or on deck, this highly inflammable liquid, which was far more dangerous than other stores of this class, had been sent in freight-ships as common cargo, being stored in the hold without any precautions. A stone jar which was advised as containing a supply had arrived at its destination in the Pacific quite empty, the contents having leaked out and evaporated on the passage out, so that the vessel carrying it had been unsuspectedly exposed to very great danger.

The disaster on board the *Triumph*, combined with the fact that this xerotine siccative had been issued to H.M. ships generally, the authorities and officers of the navy having been in ignorance as to its dangerous nature, re-directed official attention to the loss of the *Doterel* on April 26, 1881, while at anchor off Sandy Point, by an explosion, or rather by two distinct explosions following each other in very rapid succession, which caused the death of eight officers and 135 men, there being only twelve survivors of the crew. The inquiry by court-martial into the catastrophe had led to the conclusion that the primary cause of the destruction of that vessel was an explosion of gas in the coal-bunkers, caused by disengagement of fire-damp from the coal with which these were in part filled. Its distribution through the air in the bunkers and in air-spaces adjoining the ship's magazine, was believed to have taken place to such an extent as to produce a violently explosive mixture, and that this had become accidentally inflamed, causing a destructive explosion, which was followed within half a minute by the much more violent explosion of the ship's magazine containing four or five tons of powder, to which the flame from the exploding gas-mixture had penetrated.

The circumstances elicited by the inquiry, coupled with the information, relating to explosions known to have occurred in coal-laden ships, which had been collected by a Royal Commission in 1876 (of which the lecturer was a member), combined to lend a considerable amount of probability to the view adopted by the court-martial in explanation of an accident for which there appeared to be no other reasonable mode of accounting.

The conclusion arrived at led to the appointment of a committee under the presidency of Admiral Luard (of which Professor Warington Smyth and the lecturer were members) to inquire into the probabilities of coal-gas being evolved, and of an explosive gas-mixture accumulating in consequence in the coal-bunkers of ships of war, and into the possible extent and nature of damage which might be inflicted upon ships of war by explosions due to the ignition of such accumulations. The committee were also instructed, in the event of their finding that H.M. ships were liable to exposure to danger from such causes, to consider and devise the means best suited for preventing dangerous accumulations of gas in the coal-bunkers as distributed over the various parts of the ship in the different classes of vessels composing the Royal Navy.

The committee instituted a very careful inquiry, and a series of experimental investigations, including the firing of explosive gas-mixtures, in large wrought-iron tanks in the first instance, and afterwards in one of the large bunkers, empty of coal, in an old man-of-war, which afforded some comparison with the condition, as regards the relative strength or powers of resistance of the surroundings, and with the position, relatively to the ship's magazine, of the particular bunker in the *Doterel* in which it was thought the explosion might have originated. The results of these experiments could not be said to do more than lend some amount of support to the belief that effects of the nature of those ascribed to the first explosion in the *Doterel* might have been produced by the ignition of a powerfully explosive gas-mixture, contained in the middle- or athwart-ship's bunker of the ship. The committee's experimental investigations for ascertaining the best general method of securing the efficient ventilation of the coal-bunkers in different classes of men-of-war was, however, of considerable advantage in leading to the general adoption of arrangements in H.M. ships whereby the possible accumulation in the bunkers of gas which may be liable to be occluded from coal after its introduction into them is effectually prevented, and the occurrence of the kind of accident guarded against, of which there are several on record, due to the ignition of explosive mixtures which have been produced in coal-bunkers.

Although the inquiry instituted by the court-martial in August 1881, into the loss of the *Doterel* was apparently very exhaustive, some significant facts connected with the existence of a supply of xerotine siccative in the ship, which appear to have had a direct bearing upon the occurrence of the disaster, only came to light accidentally in January 1882. A caulker formerly on the *Doterel*, but then employed in the *Indus*, recognised, while some painting was being done in that ship, a peculiar odour (as he called it, "the old smell") which he had noticed in the lower part of the *Doterel* the night before the explosion; on inquiry as to the material which gave rise to it, he learned that it was due to some of the same material, xerotine siccative, that had caused the explosion in the *Triumph*. Upon this being communicated to the authorities, an official inquiry was directed to be held, and it was then elicited that the very offensive smell due to the crude petroleum spirit of which this xerotine siccative mainly consisted had been observed not only by this man (who in his evidence before the court-martial had not alluded to the circumstance), but also by several others in the *Doterel*, between decks, the night before the explosion; that, on the following day, a search was made for the cause of the odour, and that a jar containing originally about a gallon of the fluid, which was kept in a space at the bottom of the foremast together with heavy stores of various kinds, was found to have been cracked, the principal portion of its contents having leaked out into the bottom of the ship. The cracked jar was handed up to the lower deck with the siccative still leaking from it, and orders were given to throw it overboard on account of the bad smell which it emitted; this was done within a very few minutes after the jar had been removed, and the first explosion occurred almost directly afterwards. Instructions had been given to clear up the leakage from the jar after the hatch of the mast-hole had been left off a little time, and it appeared that a naked candle had been given to the man who handed the jar up out of the small store-hold described by that name. There appears very little room for doubt that an explosive mixture of the vapour and air had not only been found in the particular space where the jar was kept, but that it had also extended through the air-spaces at the bottom of the ship towards and underneath the powder-magazine, so that even the air in the latter may have been in an explosive condition, as many hours had elapsed between the time when the smell of the petroleum spirit-vapour was first noticed and when the first explosion occurred.

The special committee which had inquired into the possibility of the occurrence of a violent gas explosion in the coal-bunkers of the *Doterel*, was directed to institute experiments with a view of ascertaining whether the vapour evolved by this xerotine siccative would, in the circumstances indicated by the official inquiry, have furnished an explosive gas-mixture possessing sufficient power to have produced the effects resulting from the first explosion on the *Doterel*, and to have exploded the powder magazine. A preliminary experiment showed that when a small quantity of the liquid was spilled at one extremity of a wooden channel 7 feet long and 2·5 inches by 3 inches in section, the vapour had diffused itself in the space of three minutes throughout the channel to such an extent, that, on a light being applied at one end, the flame travelled along very rapidly to the other end, igniting a heap of gunpowder which had been placed there. Some of the liquid was also spilled upon the bottom of a very large sheet-iron tank, and after this had remained closed for about twenty-four hours, being exposed on all sides to the cool air of an autumn night, and therefore not under conditions nearly so favourable to evaporation as those obtaining in the hold of a ship, the application of flame produced an explosion of such violence as to tear open the tank. Experiments were also made with the liquid in an old man-of-war, under conditions somewhat similar to those which existed in the *Doterel*, and destructive effects were obtained of a nature to warrant the conclusion that the first explosion in the *Doterel* might have been due to the ignition of an explosive mixture of the air in the confined space at the bottom of the ship, with spirit vapour furnished by the liquid which had leaked out of the jar.

It is very instructive, as indicating the manner in which volatile liquids of this class may, if their nature be unsuspected, be the causes of grave disasters, to note that, while stringent regulations apply and are strictly enforced in our men-of-war in connection with the storage and treatment of explosives and inflammable bodies carried in the ship, the introduction into the service of this highly volatile liquid, and its supply to ships in small quantities, was speedily followed by two most calamitous accidents because the material was only known under the disguise of a name affording no indication of its character. Its dangerous nature had consequently escaped detection by the officials through whose hands it had passed, the makers of the preparation having, in a reprehensible manner which cannot but be stigmatised as criminal, withheld the information which most probably would have, at the outset, acted as a prohibition to the adoption of this material by the Admiralty for use in ships, or which would, at any rate, have led to the adoption of very special precautions in dealing with this material.

Although not initiated, nor attended, by an explosion, the accident which in December 1875 caused the loss, by fire, of the training-ship *Goliath* off Grays (near Gravesend) and the death of several of the boys by drowning, claims notice as an illustration of the facility with which, by heedlessness, or inattention to obvious precautions, accidents may be brought about in the use as an illuminating agent of mineral oil or petroleum, even where these are of such low volatility, or high "flashing point," as to entitle them to be considered as safe, under all ordinary conditions, as vegetable or animal oils. The evidence elicited at the coroner's inquest showed that one of the boys of the *Goliath*, whose duty it was, at the time, to trim the lamps used in the ship, to place them in position and remove and extinguish them in the morning, and to whom this work had been but recently allotted, let fall a lamp which, after having lowered the flame, he had carried from its assigned position into the lamp- or trimming-room, and which he could hold no longer on account of its heated state. The heated oil was scattered upon the floor and was apparently at once inflamed by the burning wick of the lamp; the floor of the room was, it appears, much impregnated with oil which had been let drop from time to time by lads employed upon the work of lamp trimming; hence the flame attacked the apartment generally with considerable rapidity, and a wind blowing at the time caused the fire to spread through the vessel so very quickly as to compel many of those composing the crew to jump overboard, and to render the rescue of the boys from burning or drowning a difficult matter. The occurrence of this accident was made the occasion, in some of the public papers, to decry petroleum oil as a dangerous illuminating agent, although it was proved that the particular oil used at the time when the fire occurred had so unusually high a flashing point that the consequent inferiority of its burning quality had been made the subject of complaint. This low volatility of the oil has been occasionally regarded as one very important element of safety in reference to its employment in lamps, but the lecturer will presently have to refer to circumstances which do not altogether substantiate this view. At any rate, however, although the heated oil which was spilled on to the floor from the lamp was in a condition favourable to immediate ignition by the burning wick, it is not at all likely that the fire would have extended almost at once with uncontrollable violence, especially in face of the excellent discipline and arrangements in case of fire which were shown to have existed in the *Goliath*, if the scrupulous cleanliness and care had been enforced which were essential in a room where lamp filling and trimming were regularly carried out, and where it was necessary to keep some supply of oil for current consumption. Instead of this, the floor and probably therefore other parts of the room appear to have been in a condition most favourable to the rapid propagation of the flame; moreover, the evidence as to proper care having been taken to keep the supply of oil required for current use in such a way as to guard against its being accidentally spilled, or to impress the boys employed upon the work with the great importance of care and cleanliness, was by no means satisfactory, and there can be little doubt that this catastrophe has to be classed among the numerous accidents of a readily avertible kind which have contributed to lead the public to form an exaggerated estimate of the dangerous character of petroleum oil as an illuminant.

The employment of liquid hydrocarbons as competitors with animal and vegetable oils in lamps for domestic use is of comparatively recent origin, although petroleum or mineral naphtha in its crude or native conditions was used at a very early date in Persia and in Japan, in lamps of primitive construction, while in Italy it was similarly employed about a century ago.

The application of the most volatile products of coal distillation to illuminating purposes in a crude way appears to have originated, so far as Great Britain is concerned, with the working of a patent taken out by Lord Dundonald in 1781, for the distillation of coal, not with a view to producing gas, but for the production of naphtha, brown or heavy oil, and tar.

In 1820, at about the time when gas-lighting was being established in London, his successors sold coal-naphtha in the metropolis for illuminating purposes; but the first really successful introduction of naphtha as an illuminating agent was made by Mr. Astley shortly afterwards, through the agency of the so-called Founders blast-lamp, which came into use for workshops and yards in factories, and of the naphtha lamp of Read Holliday of Huddersfield, with which we are well acquainted to this day, as, although it never became a success, for internal illumination of houses, it still continues in extensive use almost in its original form, by itinerant salesmen and showmen.

In the Founders lamp a current of air, artificially established, was made to impinge upon the flame and thus to greatly assist the combustion of the crude heavy oil used in it.

In the Holliday naphtha lamp the spirit finds its way slowly from the reservoir through a capillary tube to a small chamber placed at a lower level, which has a number of circumferential perforations, and is in fact at the same time the burner of the lamp and the vapour-producer which furnishes the continuous supply of illuminant, the liquid supplied to the chamber being vaporised by the heat of the jets of flame which are fed by its production.

Between 1830 and 1850 the knowledge of the production not only of oils but also of paraffin, by the distillation of coal or shale, became considerably developed by Reichenbach, Christison, Mitscherlich, Kane, du Boisson and others, and the practical success attained by the latter was soon eclipsed by that of Mr. James Young, who after establishing oil distillation at Alfreton from the Derbyshire petroleum, began to distil oils from the Bathgate mineral in 1850, and soon developed this industry to a remarkable extent.

The first lamps for burning liquid hydrocarbon which competed for domestic use, in this country, with the superior kinds of lamps, introduced after 1835, in which animal or vegetable oils were burned (solar lamps and moderator lamps), were the so-called camphine lamps (known as the Vesta and Paragon lamps) in which carefully rectified oil of turpentine was used. They gave a brilliant light, but soon acquired an evil reputation as being dangerous and liable, upon the least provocation, especially if exposed to slight draughts, to fill the air with adhesive soot-flakes.

After a time Messrs. George Miller & Co. of Glasgow (who held for a time the concession of the products manufactured by Mr. Young) tried with some amount of success to use the lighter products from the boghead mineral in the camphine lamp, but the chief aim of Mr. Young appears to have been to produce the heavier oil suitable for lubricating purposes, the light oil or naphtha meeting with an indifferent demand as a solvent, in competition with coal-tar naphtha, in the manufacture of indiarubber goods. He, however, himself used the mineral oil produced at Alfreton in Argand lamps in the earliest days of his operations; a small sale of the Bathgate oil took place about 1852–3 for use in Argand lamps, and the earliest description of lamp employed in Germany, where the utilisation of mineral oil as a domestic illuminant was first developed, appears to have been of the Argand type.

In 1853 a demand sprang up for the lighter paraffin oils in Germany. For three or four years previously, a burning oil was distilled from schist or brown coal at Hamburg by a Frenchman named Noblée, who gave it the name of *photogene.* The existence in Glasgow of a considerable supply of the oils became known to a German agent, and after they had been exported from Glasgow to Hamburg for a considerable time it was found that the chief purchaser was Mr. C. H. Stobwasser of Berlin, who appears to have originated the really successful employment of mineral oils in lamps for domestic use, and to have been the first to bring out the flat-wick burners for these oils. After a time Messrs. Young discovered the destination of their oil, and, having brought over a number of German lamps, for which a ready sale was found, commenced the lamp manufacture upon a large scale, and rapidly developed the trade in mineral (or paraffin) oil for burning purposes, which attained to great importance some time before the American petroleum oils entered the market. In 1859 a firm in Edinburgh supplied Young's company with nearly a quarter of a million burners for lamps, and it was not until 1859 that the foundation of the United States' petroleum industry was laid by Colonel G. L. Drake, who first struck oil (in Pennsylvania) at a depth of 71 feet, obtaining at once a supply of 1000 gallons per day. The lamps first used in America were probably of German make, but it need hardly be said that the lamp manufacture was speedily developed to a gigantic extent in that country. Some of the earliest lamps for burning mineral oil in dwellings which were produced in Germany and in Scotland possess considerable interest as ingenious devices for promoting the perfect and steady combustion of the oil, and as attempts to dispense with the necessity of the chimney for the production of a steady light. In one of these a small lamp was introduced into the base or stand of the lamp proper, and a tube passed from over this little lamp, through the oil reservoir into the burner, so as to supply the latter with heated air. In another, a small fan or blower with simple clockwork attached, to keep it in rapid motion, is placed in the stand, and supplies the flame with a rapid current of air. Among other workers at the perfection of mineral oil lamps was the late Dr. Angus Smith, who produced a double-wick lamp some years before the beautiful *duplex* lamps were first manufactured by Messrs. Hinks. Some of the more recent American lamps exhibit decided improvements in the details of construction of the oil reservoirs, the wick-holders, and elevators, the arrangements for extinguishing the lamps, &c.

It does not come within the province of this discourse to deal with the marvellous development of the petroleum industry in America, where the region of Western Pennsylvania now furnishes about 70,000 barrels of oil per day, having up to the 1st January, 1884, yielded a total of 250,000,000 barrels. Nor would it be relevant to enter upon the equally interesting topic of the recent extraordinary progress of the same industry in the Caucasus, which is chiefly due to Messrs. Nobel Brothers, further than to refer to the fact that the Baku petroleum lamp oil, which supplies the entire wants of Russia, and is gradually obtaining a footing in Germany and even here, appears, notwithstanding its high specific gravity, to be suitable for mineral oil-lamps of the ordinary construction. This seems to be partly owing to the comparatively small proportion of lamp oil that is extracted from the crude Baku petroleum, in consequence of which the variety of hydrocarbons composing that product of distillation which is used for illuminating purposes, presents a narrower range than is the case in the ordinary American petroleum oil of commerce. It has also been established by careful observations which Beilstein has instituted, that some American oil which is specifically lighter than the Baku oil is not so readily carried up to the flame as the latter, by the capillary action of the wick. Mr. Boverton Redwood has carried out some instructive experiments, employing different kinds of wick as siphons, and measuring the quantity of different descriptions of oil drawn over in corresponding periods of time by the different wicks. These showed that the Baku kerosine was drawn over with decidedly greater rapidity than samples of American petroleum of ordinary quality, but that on the other hand, a sample of American kerosine of the highest quality exhibited a corresponding superiority over the Baku oil experimented with. The nature and behaviour of the wick plays a most important part in determining the efficiency and also the safety of a mineral oil- or petroleum-lamp, as will be presently pointed out.

Ever since paraffin or petroleum oils, which may be included under the general designation of mineral oils, first assumed importance as illuminating agents, accidents connected with their use have continued to claim prominence among those casualties of a domestic character, which tend to cast suspicion on the safety of the material dealt with, or of the method of employing it, under the ordinary conditions fulfilled by its careful use.

The employment as an illuminant of the most volatile portions of petroleum which are classed as spirit or naphtha has been chiefly limited to the wickless Holliday lamp, in which a small continuous supply to a chamber heated by the lamp flame which surrounds it, furnishes the vapour which maintains that flame, and to the small so-called sponge lamps or benzoline lamps, of which the body is filled with fragments of sponge, and which is intended to be charged only with as much spirit as the sponge will hold thoroughly absorbed; the small flame at the top of the wick-tube being fed by the gradual abstraction of the liquid from the soaked sponge, by the wick of sponge or asbestos which fills the tube. An ingenious application of naphtha as an illuminant consists in filling a reservoir with sponge fragments, kept soaked with the spirit, the vapour of which descends by its own gravity through a narrow tube at the base of the reservoir, and issues from a fish-tail burner under sufficient pressure to produce a steady flame for some time.

The only real danger which may attend the use of the little sponge lamps arises from accidental spilling of spirit used for filling them in the neighbourhood of a flame, or from carrying out the operation of filling in the vicinity of a light. Indeed, such casualties as have been attendant upon the use of petroleum spirit as an illuminant have been mainly connected with the keeping and handling of the supplies of this very volatile liquid, and are largely attributable to want of caution or to forgetfulness. The salutary regulation prescribed by law, that vessels containing the spirit shall bear a conspicuous label indicating its dangerous character, has undoubtedly operated very beneficially in diminishing the frequency of accidents with it, by constantly admonishing to caution. It is a matter for much surprise and regret that the manufacturers of a class of miners' safety lamps, consisting of modifications of well-known types, with the ordinary oil lamp replaced by the sponge lamp, in which petroleum-spirit is burned, should have allowed trade interests to induce them to mislead those who use these lamps with regard to the nature of the illuminant supplied with them, by devising a name for it which gives a false indication of its nature, being designed to create the belief that it is an article of special manufacture, allied in character to a comparatively very safe oil largely used in miners' lamps, while in reality it is a well-known article of commerce, the safe storage and use of which demand special precautions and vigilance.

The lecturer took occasion to point out here, ten years ago, that a large proportion of the accidents arising out of the employment of petroleum- or paraffin-lamps were not actually due to the occurrence of explosions. Thus the incautious carrying of a lamp, whereby the liquid is brought into contact with the warm portion of the lamp close to the burner, may give rise to a liberation of vapour which in escaping from the lamp may be ignited, causing an outburst of flame which may alarm a nervous person and cause the dropping or overturning of the lamp. The accident which occurred in some apartments in Hampton Court Palace, in December 1882, and gave rise to a somewhat alarming fire, appeared almost beyond doubt to have originated from the employment by a domestic servant of a contrivance in which petroleum spirit was used for heating water; but, as petroleum-lamps were used in the particular residence where the fire actually occurred, public correspondence ensued regarding the dangers attending the use of such lamps, although all which were known to have been on the premises were forthcoming after the fire and found to be intact. There was, at any rate, no evidence whatever adduced in support of an assumption that the casualty was due to the explosion of a lamp, and other instances might be quoted in which the breaking out of a fire, or the destruction of or injury to life, which had evidently been caused by upsetting or allowing to fall a petroleum-lamp, has been erroneously ascribed to an explosion.

There are, however, numerous casualties which have been unquestionably caused by the occurrence of explosions in lamps, and which have in many cases been followed by the ignition of the oil, and the consequent loss of life or serious injury to those in the immediate vicinity of the accident. Careful inquiries have of late been instituted into casualties of this kind, and in many instances the explosions have been distinctly traceable to some immediate cause. In the great majority of cases they occur some considerable time after the lamp was first kindled, and when the supply of oil remaining in the reservoir has been but small. Occasional examples of the reverse are however met with. Thus, last spring, a man and his young son were sitting at a table reading, his wife being also close at hand, when a paraffin lamp which had just been lighted exploded, and the room was at once set on fire by the burning oil which escaped. The husband and wife fled from the room, both being slightly injured, but the child was unable to escape from the flame, and was burned to death. The oil used in the lamp was of a well-known brand, having a flashing point ranging from 73° to 86° F., and assuming that the recently lighted lamp had been filled with oil, and was untouched at the time of the explosion, no satisfactory explanation can be given of the accident, unless perhaps the reservoir had been so completely filled with oil that the expansion of the liquid, on its becoming slightly warm, exerted sufficient force to determine the fracture of the glass at some part where a flaw or crack existed.

A lamp accident which occurred last July at Barnsbury, causing the death of a woman and her husband, appears, on the other hand, distinctly traceable to the production of an explosion in the reservoir of the lamp. The latter was stated to have been alight but a short time, when, the husband being already in bed, the wife, in her night-dress, attempted to blow out the flame of the lamp; the man heard a report, and looking towards the lamp, saw his wife in flames. He proceeded at once to her rescue and was severely burnt in extinguishing the flames in which she was enveloped. The woman died in a few hours, and the man succumbed three days later to the injuries received. There being no witness to the accident, there is no evidence against the supposition that, on the occurrence of a slight explosion in the reservoir in the lamp, the woman, having hold of it when attempting to blow it out, may have upset it, or tilted it so as to cause the oil to flow out and become inflamed. The lamp may have become fractured by the explosion; but whenever such a result has been produced the lamp had always been burning some time, so that there was considerable air-space which could be filled by an explosive atmosphere, whereas, in this case, the evidence appears positive as to the lamp having been full of oil when lighted.

In another fatal case of a lamp explosion in the same month, at Mile End, the accident was also caused by the attempt on the part of a woman to blow out the lamp before going to bed. In this case the lamp had been burning for three hours; the husband of the sufferer was in bed asleep in the room at the time, and, the woman being unable to give any account of the occurrence, the only information elucidating it was furnished by the daughter, to the effect that the lamp had been burning for three hours, and that it was the habit of her mother to extinguish the lamp by first lowering the wick and then blowing down the chimney.

Another fatal accident, caused by the explosion of a lamp, took place at Camberwell last January, and was brought about, as in the two preceding cases, by attempts to extinguish the lamp by blowing down the chimney. The husband and two sons of the sufferer were witnesses of this accident; the lamp had been burning for six or seven hours, when the woman took it in her hand, and having partially turned it down, proceeded to blow down the chimney; an explosion at once occurred, the glass reservoir was broken, and the inflamed oil flowed upon her dress, burning her most severely.

A lamp explosion which occurred last December, in a van used as a bedroom by an itinerant showman, at the so-called World's Fair held at the Agricultural Hall, Islington, and which caused the death of an infant, was of a somewhat different character to the foregoing. The lamp, which was of the duplex form and was attached to a bracket, had been alight for some hours, when a woman went, from a neighbouring van used as the dwelling room, to extinguish it. She observed that while the lamp, or wick, was only burning faintly, the oil in the reservoir was alight. She placed her apron over the top of the chimney to extinguish the lamp, when it at once appeared to explode, and the burning oil set the interior of the van on fire. The woman ran out for help, and a lad, protecting his head with his coat rushed in and brought out the infant which was lying upon the bed, and which died from injuries received. The oil used in the lamp was believed to be of high flashing point, being obtained by the retailer who supplied it, from a firm dealing in a Scotch shale oil manufactured by the Walkinshaw Company (known as an "electric light" brand). A sample of the oil, as supplied by the wholesale dealers, had a flashing point of 114° F., but a portion of the oil actually purchased by the owner of the lamp had a flashing point of only 63° F., and evidently consisted of a mixture of the heavy oil and of benzoline. The oil in question would naturally become exhausted of the volatile spirit after the lamp had burned for some time, and the flame would then have burned low in consequence of the heavy character of the residual oil; the lamp and its contents would have thus become highly heated, and some accidental disturbance of the surrounding air must have caused vapour generated from the heated oil and contained in the air-space of the reservoir, to become inflamed; the oil itself being thereby ignited. By placing her apron hastily upon the top of the chimney, the woman forced air into the reservoir, and thus either caused a slight explosion to take place, or determined the breaking of the glass by the sudden change of temperature. A lamp-accident, apparently due to the same cause, occurred quite recently in the cabin of a small steam-launch on the Medway, near Chatham.

Several cases of undoubted lamp explosions, fortunately unattended by serious consequences, have come to the lecturer's knowledge as having occurred in the billiard-rooms of barracks where petroleum or paraffin oil was employed as the illuminant. These lamps are fixed over the billiard tables, and generally speaking the rooms have top- or sky-lights. In every instance the lamp had been burning for several hours and had probably become more or less heated, especially as shades of sheet tin were placed over them as reflectors. In each case a portion of the glass reservoir was blown out by the explosion, and the oil, becoming ignited, burnt portions of the table on which it fell.

A careful investigation of accidents of which the foregoing are illustrations,* together with a critical examination of the construction of various lamps, and the results of many experiments have, up to the present time, led the lecturer and Mr. Redwood to arrive at several definite conclusions with respect to the immediate causes of lamp-explosions and to certain circumstances which may tend to favour the production of such explosions.

If the lamp of which the reservoir is only partly full of oil, be carried, or rapidly moved from one place to another, so as to agitate the liquid, a mixture of vapour and air may make its escape from the lamp in close vicinity to the flame, and, by becoming ignited, determine the explosion of the mixture existing in the reservoir. This escape may occur through the burner itself, if the wick does not fit the holder properly, or through openings which exist in some lamps in the metal work, close to the burner, of sufficient size to allow flame to pass them readily. A sudden cooling of the lamp, by its exposure to a draught or by its being blown upon, may give rise to an inrush of air, thereby increasing the explosive properties of the mixture of vapour with a little air contained in the reservoir, and the flame of the lamp may at the same time be drawn or forced into the air-space filled with that mixture, especially if the flame has been turned down, as the latter is thereby brought nearer to the reservoir. The sudden cooling of the glass, if it had become heated by the burning of the lamp, may also cause it to crack if it is not well annealed, and this cracking, or fracture, which may allow the oil to escape, may convey the idea that an explosion has taken place. If the evidently common practice is resorted to of blowing down the chimney with a view to extinguish the lamp, the effects above indicated as produceable by a sudden cooling may be combined with the sudden forcing of the flame into the air-space, and an explosion is thus pretty certain to ensue, especially if that air-space is considerable. If the flashing point of the oil used be below the minimum (73° Abel) fixed by law, and even if it be about that point or a little above it, vapour will be given off comparatively freely if the oil in the lamp be agitated, by carrying the latter or moving it carelessly; the escape of a mixture of vapour with a little air from the lamp, and its ignition, will take place more readily, but on the other hand it will probably be feebly explosive, because the air will have been expelled in great measure by the generation of petroleum vapour. If the flashing point of the oil be high, the vapour will be less readily or copiously produced, under the conditions above indicated, but, as a natural consequence, the mixture of vapour and air existing in the lamp may be more violently explosive, because the proportion of the former to the latter is likely to be lower and nearer that demanded for the production of a powerfully explosive mixture. If the quantity of oil in the lamp reservoir be but small, and the air-space consequently large, the ignition of an explosive mixture produced within the lamp will obviously exert more violent effects than if there be only space for a small quantity of vapour and air, because of the lamp being comparatively full. If the wick be lowered very much, or if for some other reason the flame becomes very low, so that it is burning beneath the metal work which surrounds and projects over the wick-holder, the lamp will become much heated at those parts, and the tendency to the production of an explosive mixture within the space of the lamp will be increased, while, at the same time, heat will be transmitted to the glass, and it will be correspondingly more susceptible to the effects described as being exerted by its sudden exposure to a draught. Experiments have demonstrated that a lamp containing an oil of high flashing point is more liable to become heated than a comparatively light and volatile oil, in consequence of the much higher temperature developed by the combustion, and of the comparative slowness with which the heavy oil is conveyed by the wick to the flame. It therefore follows that safety in the use of mineral oil lamps is not to be secured simply by the employment of oils of very high flashing point (or low volatility), and that the use of very heavy oils may even give rise to dangers which are small, if not entirely absent, with oils of comparatively low flashing points. The occurrence of such an accident as that in the training-ship *Goliath*, already referred to, which was brought about by a boy letting fall a lamp which had been alight all night, and which was so hot that he could no longer hold it, appears to be primarily ascribable to the use of an oil of very high flashing point; and the accident at the Agricultural Hall furnished another illustration of the kind of danger attending the use of such an oil.

---

* Mr. Alfred Spencer, of the Metropolitan Board of Works, has obligingly furnished me with the official details of several of the accidents above referred to. —F. A. A.

The character of the wick very materially affects not only the burning quality of the lamp, but also its safety. A loosely plaited wick of long staple cotton draws up the oil to the flame regularly and freely, and so long as the oil be not very heavy or of very high flashing point, and therefore difficultly volatisable or convertible into vapour (by so-called destructive distillation), the flame will continue to burn brightly and uniformly, with but little charring effect upon the wick; that is to say, the extremity of the latter will only be darkened and eventually charred to a distance of much less than a quarter of an inch downwards, and it will not be until the partial exhaustion of the oil-supply diminishes the size of the flame and induces the user to raise the wick, that the latter will become more considerably charred. But, if the wick be very tightly plaited, and made, as is not unfrequently the case, of a short staple cotton of inferior capillary power, the oil will be less copiously drawn up to the flame; as a consequence, the length of exposed wick will be increased by the user of the lamp, and as the evaporation of the oil will take place more slowly from each portion of the wick which furnishes the flame, the heat to which the cotton is exposed will be greater, and the charring, which is fatal to the proper feeding of the flame by destroying the porosity of the end of the wick, will take place more rapidly and to a much greater extent.

Even with wicks of the higher qualities, considerable differences exist in the rapidity with which the oil is raised to the flame. In Mr. Redwood's experiments, conducted with a specimen of English wick of good quality and with a very superior American wick, of corresponding dimensions, the quantity of oil siphoned over by the latter in a given time, was from 35 to 47 per cent. greater (according to the nature of oil experimented with) than that carried over by the English wick.

If the wick be at all damp when taken into use, its power of conveying the oil to the flame will be decidedly diminished, the capillaries of the fibre being more or less filled with moisture, and similarly, if the oil accidentally contain any water, the latter, passing into the wick, will interfere with the proper feeding of the flame. As the oil is very thoroughly filtered or strained during its transmission through the body of the wick to the flame, it is obvious that any impurities suspended in the liquid will be deposited within the wick and will gradually diminish its porosity. For this reason the same wick should not be used for a great length of time, and it is decidedly objectionable to use a much greater length of wick than is necessary to reach to the bottom of the reservoir, and to continue its use until it has become too greatly shortened by sucessive trimmings. On the other hand, the wick should always be of sufficient length to be immersed to a considerable distance in the oil. It is evident that the copious supply of oil to the flame will become reduced as the column of liquid which covers the wick in the reservoir becomes reduced in height; hence the supply of oil in the lamp should never be allowed to get very low, not only because it is undesirable to have a large air-space which may be filled with vapour and air, but also because the burning of the lamp is injuriously affected thereby.

Some lamps, of patterns first constructed in the United States, are provided with what may be called a feeding wick in addition to the wick, or wicks, which furnish the flame. This wick is generally simply suspended from the lower surface of the burner, and reaches nearly to the bottom of the reservoir, being so placed that it hangs against one flat side of the regular wick, and thus aids considerably the copious and uniform absorption of oil by the latter. In certain lamps of recent construction the reservoir which contains the main supply of oil is so arranged (upon the principle of the old study- or Queen's oil-lamp), that it regularly maintains at a uniform level the supply of oil, which surrounds the wick in a small central reservoir or cylinder, separated from the main reservoir (excepting as regards a small channel of communication) by an air-space, which presents the additional advantage of preventing the transmission of heat to the oil-vessel. This kind of lamp is constructed entirely of metal; this is the case now with a very large proportion of the lamps in use, and unquestionably adds greatly to the safety of lamps, which, if constructed of glass or porcelain, are always liable to accidental fracture, quite apart from the question of possible explosion.

It has been proved experimentally that if the reservoir of a burning lamp be warmed, so as to favour the emission of vapour into the space above the oil, and a small opening in the top of the reservoir be then uncovered, air will be drawn into the latter and form an explosive mixture with the vapour, which, escaping from the lamp close to the wick-holder, will be fired and produce an explosion in the lamp. It is an interesting illustration of the very imperfect appreciation, by some lamp designers, of the conditions which, in the construction of a lamp, secure safety or determine danger, that the reservoirs of some petroleum lamps are actually furnished with an opening in the upper surface, which is closed with a more or less badly fitting metal cap, and is intended to be used for filling the lamp with oil. Independently of the great element of danger which this fitment presents, in consequence of the obvious temptation to the users to replenish the reservoir while the lamp is actually burning, it is very likely sooner or later to be the means of admitting to the reservoir, in the manner above indicated, the supply of air necessary to determine the explosion of vapour therein existing.

Another source of danger introduced in the construction of lamps which should be sufficiently obvious, and to which reference was made when first discussing the causes of lamp explosions, consists in the provision in many lamps, of openings of considerable size close to the burner, apparently with the object of affording a passage for the air, or vapour, in the reservoir which may expand as the lamp becomes somewhat warm. Other devices with the same object in view, consisting of small channels or shafts brought up from the top of the reservoir to the seat of the lamp flame, are adopted in some American lamps. If these openings or channels were protected, in accordance with the well-known principles which govern the construction of miners' safety lamps, so as to preclude the possibility of flame passing them, they would obviously be unobjectionable, and indeed in one or two instances of modern lamps the openings which have been provided for the escape of expanding air or vapour are of such dimensions that flame could not pass. A simple arrangement which would effect the desired object with perfect safety, and would at the same time protect the lamp wicks from deterioration by the grosser impurities sometimes contained in portions of a supply of oil, is to attach to the bottom of the burner a cylinder of wire gauze of the requisite fineness (28 meshes to the inch) which would contain the wicks, and would allow the passage of air or vapour through it towards the burner, while it would effectually prevent the transmission of fire from the lamp flame to the air-space of the reservoir.

Some of the more prominent points elicited by the inquiry in progress, as to the causes of explosions in petroleum lamps, and the conditions which regulate their efficiency and safety, having now been noticed, it remains to offer a few simple suggestions, the attention to which cannot but serve to reduce the risks of accident which attend the use of petroleum and paraffin oil.

1. It is desirable that the reservoir of the lamp should be of metal. It should have no opening or feeding place in the reservoir, nor should there be any opening or channel of communication to the reservoir at or near the burner, unless protected by fine wire gauze, or packed with wire, or unless it is of a diameter not exceeding 0·04 inch.

2. The wick used should be of soft texture and loosely plaited; it should fill the entire space of the wick-holder, and should not be so broad as to be compressed within the latter; it should always be thoroughly dried before the fire, when required for use. The fresh wick or wicks should be but little longer than sufficient to reach to the bottom of the reservoir, and should never be immersed to a less depth than about one-third the total depth of the reservoir.

3. The reservoir or lamp should always be almost filled before use.

4. If it be desired to lower the flame of the lamp for a time, this should be carefully done, so as not to lower it beneath the metal-work deeper than is absolutely necessary; but it should be borne in mind that even then the combustion of the oil will be imperfect, and that vapour of unconsumed petroleum will escape, and render the lamp very unpleasant in a room.

5. When the lamp is to be extinguished, and is not provided with an extinguishing arrangement (of which many excellent forms are now applied to lamps) the flame should be lowered until there is only a flicker; the mouth should then be brought to a level with the top of the chimney, and a sharp puff of breath should be projected *across* the opening. The lamp should remain on a firm support when it is being extinguished.

The lecturer hopes that, pending the more thorough treatment of this subject by Mr. Redwood and himself when these investigations are completed, the points dealt with in this discourse which relate to accidents with petroleum lamps may, on the one hand, tend to dispel groundless alarm as to the dangerous nature of petroleum and paraffin oil as illuminants, and may, on the other hand, serve to convey some useful information respecting the causes which lead to accidents with lamps and the readiness with which they may be avoided.

[F. A. A.]

WEEKLY EVENING MEETING,

Friday, March 20, 1885.

Sir Frederick Bramwell, F.R.S. Manager and Vice-President, in the Chair.

Professor A. W. Rücker, M.A. F.R.S.

Liquid Films.

The molecules in the interior of a liquid are surrounded on all sides by others which they attract, and by which they are themselves attracted, while those on the surface have neighbours on one side only. In consequence of this difference in their surroundings there is in all probability a difference in the grouping of the interior and exterior molecules which is attended by corresponding variations in the physical properties of the liquid of which they are constituent parts. Thus it was shown by M. Plateau that the viscosity of the surface of a liquid is in general different from that of its interior. The most striking example of this phenomenon is afforded by a solution of saponine. Two per cent. of this substance dissolved in water does not effect any marked change in the properties of the great mass of the liquid, but produces a most remarkable increase in the surface viscosity, so that forces which suffice to create rapid motion in bodies which are completely immersed, fail to produce any appreciable movement if they lie in the exterior surface. The first attempt to obtain a numerical estimate of the difference of the resistances experienced by a body oscillating in turn in the interior and in the surface of the liquid was made about two years ago by Messrs. Stables and Wilson, students in the Yorkshire College. In the case of a horizontal disc suspended in water, the logarithmic decrement diminishes to about one-half as the surface is approached. In a saponine solution, on the other hand, it is 125 times greater in the surface than in the interior, and about 38 times greater in the surface than at a depth of 0·1 mm. below it. Even in the latter case the greater part of the resistance is due, not to the friction between the disc and the liquid, but to that experienced by the supporting rod in the surface, so that in all probability the surface viscosity is more than 600 times greater than that of the mass of the liquid.

The immense change in the resistance which takes place when the disc is immersed to a depth of 0·1 mm. only confirms the general opinion that any peculiarity of grouping or arrangement due to proximity to the surface extends to a very small depth. A liquid must thus be conceived as surrounded by a very thin layer or skin, the properties of which are different from that of the liquid in the interior, and to which rather than to any ideal geometrical boundary the term surface might be applied. It may, however, prevent confusion if it is called the *surface-layer*.

Many attempts have been made to measure the thickness of the surface-layer. In particular, M. Plateau studied a thinning soap film with the view of determining whether or no the pressure exerted on the enclosed air by the film when very thin is the same as when it is comparatively thick. Had any such difference been observed it might but have been tãken as *primâ facie* evidence that the tenuity was so great that all the interior portions of the film had drained away, and that the thickness did not exceed that of the two surface-layers.

This experiment has been criticised by Prof. Reinold and myself, but it is not intended in this lecture to enter upon the general question of the thickness of the surface-layer, or the interesting theoretical problems which are closely connected with it, as we are at present engaged in an investigation which we hope may throw further light upon the subject. There are, however, two preliminary questions on which we have arrived at definite conclusions.

In any experiments which have for their object the detection of small changes in the properties of a soap film as it becomes thinner, it is essential that we should be able to assert with certainty that no causes other than the increasing tenuity have been in play, by which the effect looked for might either be produced or masked. Changes in the temperature or composition of the film, must especially be prevented.

The liquid ordinarily employed for such investigations is the "liquide glycérique" of M. Plateau. In dry air some of the water of which it is in part composed would evaporate, while in moist air, in consequence of the hygroscopic properties of the glycerine, additional water would be absorbed. Though these facts were well known, and though they are evidently possible sources of error, no attempt (as far as I am aware) had been made before our own to determine what precautions it was necessary to take to prevent the results of experiments such as M. Plateau's being affected by them. The first question then that we set ourselves to answer, was—to what extent is the composition of a soap film altered by changes in the temperature or hygroscopic state of the air which surrounds it?

The method adopted in answering this inquiry was to measure the electrical resistance of soap films formed in an inclosed space containing a thermometer and hair hygrometer. If the observations led to the conclusion that the resistance of film varied inversely as its thickness, they would prove that no change in composition had taken place, and that the film at the thinnest had afforded no evidence of an approach to a thickness equal to that of the surface-layers. If the specific resistance was found to vary according to some regular law as the thickness altered, there would be a strong presumption, that the thickness was not much greater than, and was possibly even less than that of the two surface-layers. If, lastly, the changes were irregular, they might safely be ascribed to alterations in temperature or constitution.

To obtain the desired facts it was necessary (1) to devise a method of forming the films in a closed chamber, (2) to measure their thickness, and (3) to determine their electrical resistance.

The films were formed in a glass box at the lower extremity of a platinum ring which communicated by means of a tube with the outside. In the earlier experiments a cup of the liquid was raised by rackwork to the ring and then withdrawn, leaving a film behind it. The latter was blown out by air which had been dried and passed through tubes containing "liquide glycérique." When large enough it adhered to a second platinum ring placed vertically below the first, and on some of the air being withdrawn it assumed the cylindrical form.

The thickness was measured by means of the colours displayed, two independent determinations being obtained by two beams of light incident at different angles. Newton's Table of Colours was revised, and it was found that the differences between the thicknesses given by him and those determined by new experiment were far greater than the error of experiment of a single observer. Hence, if accurate measurements are required by means of Newton's scale, every experimenter must reconstruct that scale for himself.

At first the electrical resistance was determined by means of Wheatstone's Bridge. The edges of the film where it is close to its solid supports are often, however, the seat of phenomena which might affect the results. Thin rings of white or black appear which alter the resistance considerably, and which introduce errors for which it is almost impossible to make any accurate allowance. This fact, combined with the advantage of avoiding errors due to polarisation, and of being able to select any particular part of the film for examination instead of the whole, led us to adopt a different method. Gold wires attached to a movable support were thrust into the film, and the difference of potential between these when a current was passing through the film was compared with that between the extremities of a known resistance included in the same circuit.

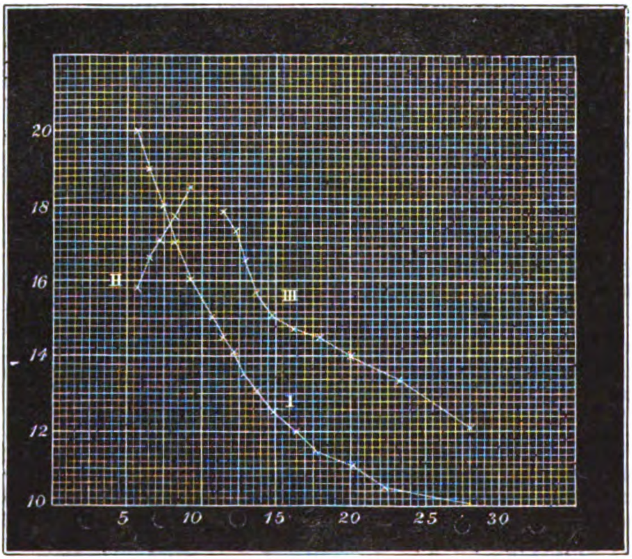
The result of these observations was to prove that the specific resistance of the films altered in an irregular manner, varying between 200 and 137 ohms per cubic c.m. A closer inspection showed that abnormal results were always accompanied by abnormal variations in the thermometer or hygrometer. When those films were selected which had been observed when such variations were especially small, it was found that the range of variation of the specific resistances was only between 137 and 146, and that the mean value was 143, that of the liquid in mass being 140·5 (at the same temperature). It was also proved that between thicknesses varying from 1370 to 374 millionths of a millimetre, no regular change in specific resistance could be detected, the actual variations lying within 2·5 per cent.

The conclusion was thus arrived at that the specific resistance of the liquid of which a soap film is formed does not differ from that of the same liquid in mass, at all events when the thickness is greater than 374×10^{-6} mm, and that comparatively small changes in the temperature or hygroscopic state of the air in contact with the film are attended with great alterations in the specific resistance, which indicate a considerable change in composition.

The method of experiment made it possible to determine the amount of this change. Solutions were made up representing "liquide glycérique" which had lost or gained given percentages of water, their specific resistances were determined at various temperatures, and approximate formulæ obtained by which the percentage of water present could be calculated if the specific resistance and temperature were known.

The results of the application of this method of analysis to a film are shown in the accompanying figure. The abscissæ represent time, the ordinates of curve I. represent the average thickness of the film.

FIG. 1.



It will be observed that the film continued to get thinner during the whole time that it was under observation. The electrical observations however, proved that at first the product of the resistance and thickness steadily increased, indicating a continuous loss of water. Curve II. shows the number of parts of water in 100 of the solution lost at the times indicated by the abscissæ. After a while a piece of blotting paper which had been hung up inside the case was moistened with water. While this was being done the observations were interrupted. On their renewal it was found that although the film thinned as steadily as before, the product of the resistance and thickness diminished instead of increasing. Curve III. shows the steady absorption of water which followed the moistening of the air. These experiments proved that it is possible for a film to undergo great changes in composition without any indication of the fact being afforded by the colours it displays. They show that if the composition of the "liquide glycérique" is to be kept constant, all change in the temperature and hygrometric state of the air must be as far as possible prevented. In later experiments this condition has been secured by placing the film box in the centre of a water tank, and by keeping an endless band of linen hung up within the case, and which dips into the liquid, continually moistened. Observations made with this apparatus show that these precautions which are certainly necessary are also sufficient.

The second point to which special attention has hitherto been given by Prof. Reinold and myself is the measurement of the thickness of very thin films. If the thickness is less than a certain magnitude, the films appear black, and thus their colour gives only a limit to and not a measure of their thickness. Black films display many remarkable properties. In general there is a sudden change in thickness at the edge of the black indicated by the omission of several colours, or sometimes of one or two orders of colours. It is only under rare conditions that a gradual change in thickness can be observed from the white to the black of the first order.

To determine the thickness of the black its resistance was measured, and the thickness calculated *on the assumption that the specific resistance was the same as that of the liquid in mass.*

The observations were made in several different ways and proved that the thickness of the black portion remains constant in any given film, however much its area may alter. Thus, in the case of a group of films measured by Wheatstone's bridge, the average resistance of a black ring 1 mm. in breadth was 1·761 megohms when the total breadth was 2 mm., and 1·760 megohms when the total breadth lay between 10 and 12 mm.

Again, the resistance of the part of the film between the needles used in the electrometer method was practically the same when the black had extended over the whole film (40 mm. long) as it had been when only the upper 11 mm. were black. The final measurement differed from the mean by only 0·1 per cent. Again, in another film the resistance of the black per millimetre remained the same to within 2·5 per cent. for an hour and a half.

On the other hand the experiments also proved that the thickness of the black was different in different films. The values found varied between $7{\cdot}2 \times 10^{-6}$ and $14{\cdot}2 \times 10^{-6}$ mm. These differences are quite outside the possible error of experiment. If they were due to changes in the constitution of the liquid of which the films were formed, it is very improbable that the specific resistance of individual films would not have shown progressive changes. As has been stated, none such were observed. The mean thickness of the five films made of "liquide glycérique" which were observed was $11 \cdot 9 \times 10^{-6}$ mm., while that of 13 films made of soap solution without any glycerine was $11 \cdot 74 \times 10^{-6}$ mm.

The assumption made in these calculations that the specific resistance of a film, the thickness of which is ten or twelve millionths of a millimetre, is the same as that of the liquid in mass, is not justified by the previous experiments, which had proved it to hold good only to the much greater thickness of 370×10^{-6} mm. It was therefore desirable to check the results by an independent method. For this purpose 50 or 60 plane films were formed side by side in a glass tube which was placed in the path of one of the interfering beams in a Jamin's Interferential Refractometer. The compensator was adjusted so that it had to be moved through a large angle to cause one interference band to occupy the position previously held by its neighbour, i.e. to alter the difference of the paths of the interfering rays by one wave length. This angle was determined for the red light of known wave length transmitted by glass coloured with copper oxide. When the films had thinned to the black they were broken by means of a needle which had been included in the tube along with them, and which was moved, without touching the tube, by a magnet. The rupture of the films produced a movement of the interference fringes which was measured by the compensator, and from which, in accordance with well-known principles, the thickness of the films could be deduced.

The mean thickness given by seven experiments on films made of "liquide glycérique" was $10 \cdot 7 \times 10^{-6}$ mm., that obtained from nine experiments on films made of soap solution was $12 \cdot 1 \times 10^{-6}$ mm. The mean of these, or $11 \cdot 4 \times 10^{-6}$ mm., differed only by $0 \cdot 4 \times 10^{-6}$ mm. from the mean thickness deduced from the electrical experiments.

The last point to which reference is necessary is one which lies outside the main line of the enquiries above described, but which is nevertheless not without interest. In the course of the observations it was noticed that the rate of thinning of a film seemed to be affected by the passage of the electric current through it. Some experiments made on this point last year proved the fact beyond the possibility of doubt. The current appears to carry the matter of the film with it, so that it thins more rapidly if the current runs down, and less rapidly if the current runs up than if no current is passing. This may be shown as a lecture experiment.

A vertical rod which can be moved up and down by rackwork is passed through the centre of the cover of a glass film-box. To the lower extremity is attached a horizontal platinum wire, from which another similar horizontal wire is suspended by two silk fibres. A film is formed by lowering the whole into the liquid with which the lower part of the vessel is flooded. The light reflected from the film is passed through a lens, and an image formed upon a screen. When the bands of colour are seen descending from the upper part of the film a current from 50 Grove's cells is passed through it. If the current flows downwards the bands of colour move more quickly than before; if it flows upwards their motion is checked and they begin to ascend. The cause of this curious fact is still unknown. It may either be analogous to the phenomenon known as the "migration of the ions," or it may be a secondary effect due to a change in the surface tension.

The general relation of the results attained in these investigations as to the question of the size of molecules is interesting. Sir William Thomson has expressed the opinion that 2×10^{-6} mm. and 0.01×10^{-6} mm. are superior and inferior limits respectively to the diameter of a molecule. Van der Waals has been led, from considerations founded on the theory of gases, to give $0 \cdot 28 \times 10^{-6}$ mm. as an approximate value of the diameters of the molecules of the gases of which the atmosphere is composed. The number of molecules which could be placed side by side within the thickness of the thinnest soap film would, according to these various estimates, be 4, 26, and 720 respectively. The smallness of the first of these numbers, especially when it is remembered that the liquid used on some occasions was of a highly complex character, containing water, glycerine and soap, points to the conclusion that the diameter of a molecule is considerably less than 2×10^{-6} mm.

[A. W. R.]

WEEKLY EVENING MEETING,

Friday, March 27, 1885.

Sir Frederick Bramwell, F.R.S. Manager and Vice-President, in the Chair.

Victor Horsley, Esq. F.R.C.S.

The Motor Centres of the Brain, and the Mechanism of the Will.

Feeling deeply as I do the responsibility I have incurred in undertaking to address you to-night, I desire to express my regret that I cannot instead share with you the pleasure of listening to the distinguished man who has been prevented by a most painful bereavement from addressing you to-night.

My subject being the mechanism of the will, it might be asked, "What has a surgeon to do with psychology?" To which I would answer, "Everything." For without sheltering myself behind Mr. Jonathan Hutchinson's trite saying that "a surgeon should be a physician who knows how to use his hands," I would remind you that pure science has proved so good a foster-mother to surgery, that diseases of the brain which were formerly considered to be hopeless, are now brought within a measurable distance of the knife, and therefore a step nearer towards cure. Again, I would remind you that surgeons rather than physicians see the experiments which so-called Nature is always providing for us,—experiments which, though horribly clumsy, do on rare occasions, as I shall presently show you to-night, lend us powerful aid in attempting to solve the most obscure problems ever presented to the scientist.

The title I have chosen may possibly be objected to as too comprehensive; but until we are ready to admit a new terminology we must employ the old in order to convey our meaning intelligibly, although there may be coupled therewith the risk of expressing more than we desire. Thus when I speak of the mechanism of the will and the motor centres of the brain, I do not intend (as indeed must be obvious) to discuss the existence of the so-called freedom of the will, or the source of our consciousness of voluntary power.

I shall rather describe to you first the general plan of the mechanism which conveys information to our brain, the thinking organ; next the arrangement of those parts in it which are concerned with voluntary phenomena; and finally I shall seek to show by means of experiment that the consciousness of our existing as single beings, the consciousness of our possessing but one will as people say, while at the same time we know that we possess a double nervous system, is due to the fact that pure volition is dependent entirely on the exercise of the attention which connotes the idea of singleness. Consequently that it is impossible to carry out two totally distinct ideas at one and the same moment of time, when the attention must of course be fully engaged upon each.

I fear that in making my argument consecutive, I shall have to pass over very well-beaten paths, and so I must ask your patience for a few moments while I make good my premisses.

The nervous system, which in man is composed of brain, spinal cord, nerves, and nerve-endings, is arranged upon the simplest plan, although the details of the same become highly complex when we arrive at the top of the brain.

At the same time, while we have this simple plan of structure, we find that there is also a fundamental mode of action of the same—a mode which is a simple exposition of the principle, no effect without a cause—a mode of action which is known as the phenomenon of simple reflex action.

The general plan of the whole nervous system is illustrated by this model. Imbedded in the tissues all over the body, or highly specialised and grouped together in separate organs, such as the eye or ear, we find large numbers of nerve-endings, that is, small lumps of protoplasm from which a nerve fibre leads away to the spinal cord and so up to the brain.

These nerve-endings are designed for the reception of the different kinds of vibration by which energy presents itself to us. As the largest example of these nerve-endings, let me here show you one of the so-called Pacinian bodies, or more correctly Marshall's corpuscles, for Mr. John Marshall discovered these bodies in England before Pacini published his observations in Italy. Here you see one of these small oval bodies arranged on the ends of one of the nerves of the fingers, and here you see the nerve fibre ending in the little protoplasmic bulb which is protected by a number of concentric sheaths.

Pressure or any form of irritation of this body at the end of the nerve fibre causes a stream of nerve energy to travel through the spinal cord to the brain, and so we become conscious that something is happening to the finger.

Here in this section of the sensitive membrane of the back of the eye, the retina, you see a similar arrangement, only more complicated, namely, nerve fibres leading away from small protoplasmic masses which possess the property of absorbing light and transforming it into nerve energy. It is this transformation into nerve energy of heat, light, pressure, &c., which it seems to me should alone be called a sensation, irrespective of consciousness. And in fact we habitually say we *feel* a sensation. The terms feeling and sensation, however, are frequently used as interchangeable expressions, although, as I shall show you directly, "feeling" is the conscious disturbance of a sensory centre in the surface of the brain, and in fact feeling is the conscious perception of sensations. This distinction between feeling and sensation, if dogmatic, will save us from dispute as to the meaning of the word sensation; and further, the distinction is one, as I have just shown, which is justified by custom.

Now the nerve fibre which conveys the energy of the sensation is a round thread of protoplasm which in all probability connects the nerve-ending with a sensory corpuscle in the spinal cord. These nerve fibres running in nerves are white, whereas, as you know, protoplasm is grey. They are white because each is insulated from its fellow by a white sheath of fatty substance, just as we protect telegraph wires with coatings. It is not stretching analogy too far to say that nerve force may probably escape unless properly insulated.

In consequence of the fibres being covered with these white sheaths, they form what is called the white matter of the brain; while the nerve centres are greyish, and therefore form what is called the grey matter of the brain, so that the grey matter receives and records the messages conveyed to it by the white insulated fibres.

From the sensory corpuscle, which is a small mass of protoplasm provided with branches connecting it to neighbouring corpuscles, the nerve energy if adequate passes along a junction thread of protoplasm to a much larger corpuscle, which is called a motor corpuscle, and the energy of which when liberated by the nerve impulse from the sensory corpuscle is capable of exciting muscles into active contraction. These two corpuscles form what is called a nerve centre.

Not only are the motor corpuscles fewer as well as much larger than the sensory ones, but also the nerve fibres which go out from them are larger too. In fact, it would seem as if we had another close analogy to electrical phenomena; for here, where we want a sudden discharge of a considerable intensity of nerve force, we find to hand a large accumulator mechanism and a large conductor, the resistance of which may justly be supposed to be low. Finally, the motor nerve-fibre terminates in a protoplasmic mass which is firmly united to a muscle fibre, and which enables the muscle fibre to contract and so cause movement of one or more muscles. Now, with this idea of the general plan on which the whole nervous system is constructed, you will understand that muscular action, i. e. movement, will occur in proportion to (1) the intensity of the stimulation of the sensory corpuscle, and (2) the resistance in the different channels. When a simple flow through the whole apparatus occurs, it is called a simple reflex action, and this was discovered in England by Dr. Marshall Hall.

To recapitulate: a nerve centre, theoretically speaking, we find to consist of a sensory corpuscle on the one hand and a motor corpuscle on the other, both these being united by junction threads or commissures. To such a centre come sensations or impressions from the nerve-endings, and from such a centre go out impulses which set the muscles in action.

I have dwelt thus at length on this most elementary point, because it appears to me that in consequence of the rapidity with which function is being demonstrated to be definitely localised in various portions of the cerebral hemispheres, we are in danger of losing sight of Dr. Hughlings Jackson's grand generalisations on nerve function, and that we are gradually inclining to the belief that the function of each part is very distinct, and therefore can most readily act without disturbing another part.

In fact, we are perhaps drifting towards the quicksands of spontaneity, and disregarding entirely the facts of every-day life which show that every cycle of nerve action includes a disturbance of the sensory side as well as the active motor agency. Did we in fact admit the possibility of the motor corpuscle acting *per se*, and in the absence of any sensory stimulation, we should again be placed in the position of believing that an effect could be produced in the absence of a cause.

For these reasons such a centre has been termed kinæsthetic or sensori motor, and such centres exist in large quantities in the spinal cord, and they perform for us the lower functions of our lives without arousing our consciousness or only the substrata of the same.

But now, turning to the brain, although I am extremely anxious to maintain the idea just enunciated that when discussing the abstract side of its functions we should remember the sensori motor arrangement of the ideal centre, I shall have to show you directly that the two sides, namely, the sensory and motor in the brain are separated by a wide interval, and that in consequence we have got into the habit of referring to the groups of sensory and motor corpuscles in the brain as distinct centres. I trust you will not confuse these expressions, this unfortunately feeble terminology, and that you will understand, although parts may be anatomically separated and only connected by commissural threads, that functionally they are closely correlated.

In consequence of the bilateral symmetry of our bodies we possess a double brain—a practically symmetrical arrangement of two intimately connected halves or hemispheres which, as you know, are concerned with opposite sides of the body, for the right hemisphere moves the left limbs, and *vice versâ.*

For my purpose it will be sufficient if we regard the brain as composed of two great collections of grey matter or nerve corpuscles which are connected with sensory nerve-endings, with muscles, and intimately with one another.

In this transverse section of a monkey's brain, which is stained dark blue to show up its component parts, you will see all over the surface a quantity of dark grey matter, which is simply the richly convoluted surface of the brain cut across. Observe it is about $\frac{1}{4}$ inch deep, and from it lead downwards numerous white fibres down towards the spinal cord. The surface of the brain, the highest and most complicated part of the thinking organ, is called the cortex, bark, or rind, and in it are arranged the motor centres I am about to describe. These white fibres coming away from it to the cord, not only are channels conveying messages down to the muscles, but also carrying messages from the innumerable sense corpuscles all over the body.

So much for one grey mass of centres. Now down here at the base of the brain you see two lumps or masses of the same nature, and these are called therefore the basal ganglia or grey masses. Since they are placed at the side of the paths from the cortex, and undoubtedly do not interfere with the passage of impulses along those paths, we may put them aside, remembering that they probably are concerned with low actions of the nervous system, such as eating, &c., which are popularly termed automatic functions.

In this photograph of a model made by Professor Aeby, of Berne, you see represented from the front the two cerebral hemispheres with the centres in the cortex as little masses on the surface, and the basal ganglia as darker ones at the bottom, while leading from them down into the spinal cord are wires to indicate the channels of communication.

Note in passing that both hemispheres are connected by a thick band of fibres called the corpus callosum. It is, I believe, the close union thus produced between the two halves that leads in a great measure (though not wholly) to consonance of ideas.

The arrangement of the fibres will be rendered still clearer by this scheme, in which the cortex is represented by this concave mass, and the fibres issuing from the same by these threads.

The basal ganglia would occupy this position, and they have their own system of fibres.

I will now leave these generalisations, and explain at once the great advance in our knowledge of the brain that has been made during the last decade. The remarkable discovery that the cortex or surface of the brain contained centres which governed definite groups of muscles, was first made by the German observers Hitzig and Fritsch; their results were, however, very incomplete, and it was reserved for Professor Ferrier to produce a masterly demonstration of the existence and exact position of these centres, and to found an entirely new scheme of cerebral physiology.

The cortex of the brain, although it is convoluted in this exceedingly complex manner, fortunately shows great constancy in the arrangement of its convolutions, and we may therefore readily grasp the main features of the same without much trouble.

From this photograph of the left side of an adult human brain, you will see that its outer surface or cortex is deeply fissured by a groove running backward just below its middle, which groove is called the fissure of Sylvius, after a distinguished mediæval anatomist. This fissure if carried upwards would almost divide the brain into a motor half in front and a sensory half behind.

Of equal practical importance is another deep fissure which runs at an open angle to the last, and which is called the fissure of Rolando,

Rolando being another pioneer of cerebral topography. Now it is around this fissure of Rolando that the motor side of the centres for voluntary movement is situated; and when this portion of the cortex is irritated by gentle electric currents, a constant movement follows according to the part stimulated.

Because of their upward direction, the convolutions bounding the fissure of Rolando are called respectively the ascending frontal and ascending parietal convolutions.

Now here, at the lowest end of the fissure of Rolando, we find motor areas for the movement of both sides of the face, that is to say, that as regards this particular piece of the cortex, it has the power of moving not only its regular side of the face, the right, but also the left—that in fact both sides of the face move by impulse from it.

Higher up we find an area for movement of the opposite side of the face only. I reserve for a moment the description of this portion of the brain, and pass on to say that above these centres for the face we find the next is for the upper limb, and most especially the common movement of the upper limb, viz. grasping, indeed the only forward movement which the elbow is capable of, namely flexion. The grasping and bringing of an object near to us is the commonest movement by far, and we find here that this centre is mainly concerned in it. Behind the fissure of Rolando, Dr. Ferrier placed the centres for the fingers.

Next above the arm area is a portion of the cortex which moves the lower limb only, and in front of this again is an area for consonant action of the opposite arm and leg.

Let me here remind you that this being the left hemisphere, these are the centres for movement of the opposite, that is the right limbs, and that in the other hemisphere there are corresponding areas for the left limbs.

Thus here we have mapped out those portions of the cortex which regulate the voluntary movement of the limbs. So far I have omitted mention of the muscles of the trunk, namely, those which move the shoulders, the hips, and bend and straighten the back. Dr. Ferrier had shown that there existed on the outer surface of the cortex, here, a small area for the movement of the head from side to side.

Professor Schäfer and myself have found that the large trunk muscles have special areas for their movement, ranged along the margin of the hemisphere, and dipping over into the longitudinal fissure. Thus all the muscles of the body are now accounted for, and I will first draw special attention to the fact that they are arranged in the order, from below upwards, of face, arm, leg, and trunk.

The consideration of this very definite arrangement led Dr. Lauder Brunton to make the ingenious suggestion that it followed as a necessary result of the progressive evolution of our faculties. For premising in the first place from well-ascertained broad generalisations that the highest centre, physically speaking, is also the highest functionally and most recent in acquirement, we find that the lowest is the face, and then we remember that the lowest animals simply grasp their food with their mouth. I imagine it is scarcely necessary for me to repeat the notorious confession that our faculties are arranged for the purpose of obtaining food as the primary object of what is called bare existence.

Proceeding upwards in the scale of evolution, we next find animals which can grasp their prey and convey it to the mouth, and so we find next to the face area evolved that for the arm.

And so on, the next step would be the development of the legs to run after the prey, and here is the leg centre; while finally, the trunk muscles are dragged in to help the limbs more effectually.

To my mind this idea receives overwhelming support from the consideration of the fact that the higher our centres are, the more they require education; the infant, for instance, in a few days shapes its face quite correctly to produce the food-inspiring yell, yet takes months or years to educate its upper limbs to aid it in the same laudable enterprise. Finally, what terrible probation some people pass through at the hands of dancing-masters before their trunk muscles will bend into the bow of politeness.

Now to return to the lower end of the fissure of Rolando, to the areas for movements of the face, it was long ago pointed out by the two Dax's and Professor Broca that when this portion of the brain immediately in front of the face area was destroyed, that the person lost the power of articulate speech, or was only capable of uttering interjections and customary "strange oaths."

In fact this small portion of the left side of our brains (about 1½ square inches) is the only apparatus for expressing our thoughts by articulating sounds, and note particularly that it is on the left side. The corresponding piece on the right side cannot talk as it were. This remarkable state of things is reversed in left-handed people. In these the right hemisphere predominates; and so we find that when this portion was diseased, there followed aphasia, as it is called. While, however, the right side customarily says nothing, it can be taught to do so in young people, though not in the aged.

Before leaving these motor areas, let me repeat, by way of recapitulation, that the only truly bilaterally acting areas are those for the lower facial and throat muscles. This is a most important fact, for the idea has recently been propounded that both sides of the body are represented in each motor region of each hemisphere. That is to say, each motor area has to do with the movements of both upper limbs, for example. In support of my contention that this is not in accordance with clinical facts, let me here show you photographs of the brain of a man who was unfortunate enough to suffer destruction of the fibres leading from one motor area. Here you see a puncture in the brain which has caused hæmorrhage beneath the fissure of Rolando and the motor convolutions in front and behind it.

In this transverse section of the same spot you see that the hæmorrhage has ploughed up the interior of the brain. Here is the cortical grey matter, but its fibres leading down to the muscles are all destroyed.

Now in examining this patient I asked him to move his left arm or leg; he was perfectly conscious, and understanding the question, made the effort as we say, but no movement occurred.

Now if both sides of the body are represented in each hemisphere, it seems to me that such a case would be impossible, or at least that; a little practice would enable the other hemisphere to do the work but all clinical facts say that, once destroyed, the loss is never recovered.

If we examine this motor region of the cortex with the microscope, we of course find these large corpuscles, which we have learnt are those which alone give energy to the muscles.

But you must not imagine that the motor region consists solely of these corpuscles. On the contrary, as you see in this diagram, we have several layers of corpuscles. I shall return to this arrangement of the corpuscles directly.

Looking back at the surface of the brain, you notice that I have only accounted for but a small portion of the cortex. Dr. Ferrier was the first to show that the portion of cortex which perceived (and I use the word in its strictest sense) the sensation of light was this part, and it is therefore called the visual centre or area. From recent researches it would appear that we must give it the limits drawn on this diagram. Below it we find the centre for hearing.

Thus we know where two sense perceptive centres are situated.

Microscopical investigation shows that this sensorial portion of the cortex is very deficient in large corpuscles, and is correspondingly rich in small cells. Here in this diagram you see these two kinds of structure in the cortex cerebri. Note the greater number and complication of the small corpuscles in the sensory part of the cortex, and the comparatively fewer though much larger corpuscles in the motor region.

It seems to me that several beliefs are justified by these facts.

In the first place, the movements produced by the action of these motor centres are always the same for the same centre; consequently it has only one thing to do, one idea as it were. Thus, for instance, bending of the arm; this action can only vary in degree, for the elbow will not permit of other movements. Hence we may look upon it as one idea. Now observe that where one idea is involved, we have but few corpuscles.

Next consider the multitude of ideas that crowd into our mind when we receive a sensation. One idea then rapidly calls up another, and so we find anatomically that there are a corresponding much greater number and complication of nerve corpuscles.

To sum up, I believe we are justified in asserting that where in the nervous system a considerable intensity of nerve energy is required (e. g. for the contraction of muscles) you find a few large corpuscles and fibres provided, and that where numerous ideas have to be functionalised, there numerous small corpuscles are arranged for the purpose.

But now the special interest attaching to the sensory perceptive areas is that they, unlike the motor areas, tend to be related to both sides of the body. With our habit of constantly focussing the two eyes on one object, it will strike you at once that habitually we can only be attentively conscious of one object at a time, since both eyes are engaged in looking at it, and as you know we cannot as a matter of fact look at two things at once.

Hence I take it, both sensory perceptive centres are always fully occupied with the same object at the same moment, and that therefore we have complete bilateral representation of both sides of the body in each hemisphere. As a further consequence, each sensory perceptive area will register the idea that engaged it; in other words, both centres will remember the same thing. Thus it happens that each sensory area can perform the duty of the other, and therefore it is a matter of comparative indifference whether one is destroyed or not, and as a matter of fact when this happens we find that the person or animal recognises objects as they actually are, and in fact has no doubt as to their nature. Here you see anatomically the reason of this peculiarity is found to be that the optic or seeing nerves cross one another incompletely in going to each hemisphere, and thus each sensory centre represents half of each eyeball.

I must pass rapidly to the description of the rest of the surface of the brain—the hinder and front ends. At the outset I must admit that all our knowledge concerning them is very hypothetical in the absence of positive experimental results.

This much we can say, that they are probably the seats of intellectual thought, for many reasons which I have not time to detail. Further we know that these intellectual areas are dependent for their activity entirely on the sensory perceptive centres, for the dictum that there is no consciousness in the absence of sensory stimulation is very well established, as I shall now show you, however astounding it may appear. In the first place, you will remember that when we wish to encourage that natural loss of consciousness which we call sleep, we do all we can to deprive our sense organs and areas of stimulation; thus we keep ourselves at a constant temperature, we shut off the light, and abolish all noises if we can. But a most valuable observation was made a few years ago by Dr. Strümpell, of Leipzig, who had under his care a youth, the subject of a disease of the brain, &c., which while destroying the function of one eye and ear, besides the sensibility to touch over the whole body, still left him when awake quite conscious and able to understand, &c., using his remaining eye and ear for social intercourse. Now when these were carefully closed he became unconscious immediately, in fact slept, and slept until he was aroused again, or woke naturally as we say after some hours.

Hence the higher functions of the brain exercised when that organ is energising the reasoning of the mind, are absolutely dependent upon the reception of energy from the sense perceptive areas.

But my only point with reference to this part of the brain is to attempt to determine how far they are connected with the motor centres in the performance of a voluntary act. With the mechanism of choice and deliberate action I have nothing to do, but there can be no doubt that the part of the brain concerned in that process of the mind is directly connected with the motor region, as indicated on this diagram, to which I would now return. From what I have here written you read, arranged schematically, the psychical processes which for the sake of argument we may assume are carried on by the mind in these portions of the cortex.

I wish to point out that we have structurally and physiologically demonstrated with great probability the paths and centres of these psychical actions. There is no break; the mere sight of an object causes a stream of energy to travel through our sense areas, expanding as it goes by following the widening sensory paths were represented, and at the same time we feel our intellect learns that new ideas are rising up and finally expand into the process of deliberate thought, concerning which, all we know is from that treacherous support, namely introspection.

Then comes impulses to action, and these follow a converse path to the receptive one just described; the nerve energy is concentrated more and more until it culminates in the discharge of the motor corpuscles. We might represent the whole process of the voluntary act by two fans side by side, and the illimitable space above their arcs would serve very well to signify the darkness in which we sit concerning the process of intellectual thought.

What I have hastily sketched is the outline of the process of an attentive or voluntary act. I say attentive advisedly, for I wish now to put forward the view that the proper criterion of the voluntary nature of an act is not the mere effort that is required to perform it, but is the *degree to which the attention is involved.* The popular view of the volitional character of an act being decided by the effort to keep the action sustained is surely incomplete, for in the first place we are not seeking to explain our consciousness of an effort, we endeavour to discover the causation of the effort. Our sense of effort only comes when the will has acted, and that same sense is no doubt largely due to the information which the struggling muscle sends to the brain, and possibly is a conscious appreciation of how much energy this motor corpuscle is giving out.

Now to give you an example. I see this tambour and decide to squeeze it, and do so. Now this was a distinctly voluntary act; but the volitionary part of it was not the effort made, it was the deliberate decision to cause the movement.

I may now point out that in this whole process we say, and say rightly, that our attention is involved so long as we are deliberating over the object, that as soon as another object is brought to us our attention is distracted, that is to say, turned aside.

All writers are agreed that the attention cannot be divided, that we really only attend to one thing at once.

It seems to me that this is so obvious as not to require experimental demonstration, but I have led up to this point because I now wish to refer to the third part of my subject, namely, the question as to whether we have a really double nervous system or not; but by way of preface let me repeat that although we may have a subconsciousness of objects and acts, that that subconscious state is true automatism, and that such automatic acts are in no sense voluntary until the attention has been concentrated upon them. For example, again I press this tambour because I desire to raise the flag, and I keep that raised while I attend to what I am saying to you. My action of keeping the flag raised is only present to my consciousness in a slight or subordinate degree, and does not require my attention, deliberate thought or choice, and therefore I repeat is not a voluntary action, in fact it could be carried on perfectly well by this lower sensori motor centre, which only now and then sends up a message to say it is doing its duty, in the same way as a sentry calls out "All well" at intervals.

But to return. In consequence of the obvious fact that we have two nerve organs, each more or less complete, some writers have imagined that we have two minds; and to the Rev. Mr. Barlow, a former Secretary of this Institution, is due the credit of recognising the circumstances which seem to favour that view. It was keenly taken up, and the furor culminated in a German writer (whose name I am ashamed to say has escaped me) postulating that we possess two souls.

Now the evidence upon which this notion rests, that the two halves of the brain might occasionally work independently of one another at the same moment, was of two kinds. In the first place it was asserted that we could do two different things at once, and in the second place evidence was produced of people acting and thinking as if they had two minds.

Now, while of course admitting that habitually one motor centre usually acts at one moment by itself, I am prepared to deny *in toto* that two voluntary acts can be performed at the same time, and I have already shown what is necessary for the fulfilment of all the conditions of volition, and that these conditions are summed up in the word attention.

Further, I have already shown that when an idea comes into the mind owing to some object catching the eye, that both sensory areas are engaged in considering it. It seems to me I might stop here, and say that here was an *à priori* reason why two simultaneous voluntary acts are impossible; but as my statements have met with some opposition, I prefer to demonstrate the fact by some experiments.

The problem, stated in physiological terms, is as follows:

Can this right motor region act in the process of volition, while at the same time this other motor area is also engaged in a different act of volition?

Some say this is possible; but in all cases quoted I have found that subconscious or automatic actions are confused with truly voluntary acts. I mean that such automatic acts as playing bass and treble are not instances of pure volition, as the attention is not engaged on both notes at once.

Consider for a moment the passage of the nerve impulses through the brain that would have to occur. At the outset we find that the sensory perceptive centres would have to be engaged with two different ideas at once; but Lewes showed long ago that introspection tells us this is impossible, that "consciousness is a seriated change of feelings," he might equally well have said ideas. And again, we know that when two streams of energy of like character meet one another, they mutually arrest each other's progress by reason of interfering with the vibration waves.

I will show directly that this is actually the case in the action of the cortex when the above-mentioned dilemma is presented to it.

The experiment I have devised for this purpose is extremely simple.

A person who is more or less ambidextrous, and who has been accustomed for a long time to draw with both hands, attempts to describe on a flat surface a triangle and circle at the same moment. I chose these figures after numerous trials as being the most opposite, seeing that in a triangle there are only three changes of movement, while in a circle the movement is changing direction every moment. To ensure the attempt to draw these figures simultaneously succeeding, it is absolutely necessary that the experimenter should be started by a signal.

When the effort is made, there is a very definite sensation in the mind of the conflict that is going on in the cortex of the brain. The idea of the circle alternates with that of the triangle, and the result of this confusion in the intellectual and sensorial portions of the brain is that both motor areas, though remembering as it were the determination of the experimenter to draw distinct figures, produce a like confused effect, namely, a circular triangle and a triangular circle. If the drawing is commenced immediately at the sound of the signal, it will be found that the triangle predominates; thus if I determine to draw a triangle with my left hand and a circle with my right, the triangle (though with all its angles rounded off) will be fairly drawn, while the circle will be relatively more altered, of course made triangular. On the other hand, if the two figures are not commenced simultaneously, it will be found that usually the one begun last will appear most distinct in the fused result, in fact will very markedly predominate.

Now the course of events in such an experiment appears to be clear.

The idea of a triangle and circle having been presented to the intellect by the sensory centres, the voluntary effort to reproduce these is determined upon. Now, if we had a dual mind, and if each hemisphere was capable of acting *per se*, then we should have each intellectual area sending a message to its own motor area, with the result that the two figures would be *distinct* and *correct*, not fused.

The other evidence that I referred to above, which is adduced in favour of the synchronously independent action of the two hemispheres, is from the account of such cases as the following. Professor Ball, of Paris, records the instance of a young man who one morning heard himself addressed by name, and yet he could not see his interlocutor. He replied, however, and a conversation followed, in the course of which his ghostly visitant informed him that his name was M. Gabbage.

After this occurrence he frequently heard M. Gabbage speaking to him. Unfortunately M. Gabbage was always recommending him to perform very outrageous acts, such as to give an overdose of chlorodyne to a friend's child, and to jump out of a second-floor window. This led to the patient being kept under observation, and it was found that he was suffering from a one-sided hallucination.

Similar cases have been recorded in which disease of one sensory perceptive area has produced unilateral hallucination.

I cannot see that these cases in any way support the notion of the duality of the mind. On the contrary, they go to show that while as a rule the sensory perceptive areas are simultaneously engaged upon one object, it is still possible for one only to be stimulated, and for the mind to conclude that the information it receives in this unusual way must be supernatural, and at any rate proceeding from one side of the body.

To conclude, I have endeavoured to show that as a rule both cerebral hemispheres are engaged at once in the receiving and considering one idea. That under no circumstances can two ideas either be considered or acted upon attentively at the same moment. That therefore the brain is a single instrument.

It now appears to me that one is justified in suggesting that our ideas of our being single individuals is due entirely to this single action of the brain.

Laycock showed that the Ego was the sum of our experience, and every writer since confirms him. But our experience means (1) our perception of ideas transmitted and elaborated by the sensory paths of the brain, and (2) our consciousness of the acts we perform. If now these things are always single, the idea of the Ego surely must also be single.

[V. H.]

GENERAL MONTHLY MEETING,

Monday, April 6, 1885.

The Hon. Sir William R. Grove, M.A. D.C.L. F.R.S. Manager and Vice-President, in the Chair.

James Stewart Hodgson, Esq.
Henry W. Wimshurst, Esq.

were elected Members of the Royal Institution.

The following arrangements for the Lectures after Easter were announced:—

Professor Arthur Gamgee, M.D. F.R.S.—Eight Lectures on Digestion and Nutrition; on Tuesdays, April 14 to June 2.

Professor Tyndall, D.C.L. LL.D. F.R.S. *M.R.I.*—Five Lectures on Natural Forces and Energies; on Thursdays, April 16 to May 14.

Professor C. Meymott Tidy, M.B. F.C.S. *M.R.I.*—Three Lectures on Poisons in relation to their Chemical Constitution and to Vital Functions; on Thursdays, May 21, 28, June 4.

William Carruthers, Esq. F.R.S.—Four Lectures on Fir-trees and their Allies, in the Present and in the Past; on Saturdays, April 18, 25, May 2, 9.

Professor William Odling, M.A. F.R.S. *M.R.I.*—Two Lectures on Organic Septics and Antiseptics; on Saturdays, May 16, 23.

Rev. C. Taylor, D.D.—Two Lectures on A lately discovered Document, possibly of the First Century, entitled 'The Teaching of the Twelve Apostles,' with Illustrations from the Talmud; on Saturdays, May 30, June 6.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Palæontologia Indica: Series XII. Vol. I. Part 4, Fasc. 3, 4. 4to. 1884.
Memoirs, Vol. XXI. Parts 1, 2. 8vo. 1884.
Records, Vol. XVIII. Part 1. 8vo. 1885.
The British Museum—Catalogue of Early English Books to 1640. 3 vols. 8vo. 1884.
Catalogue of Oriental Coins, Vol. VIII. 8vo. 1883.
Catalogue of Greek Coins: Central Greece. 8vo. 1884.
Guide to Wycliffe Exhibition. 8vo. 1884.
Guide to Reading Room and Libraries. 12mo. 1884.
The British Museum (*Natural History Department*)—Catalogue of Fossil Sponges. By G. J. Hinde. 4to. 1883.
Catalogue of Lizards. 2nd edition. By G. A. Boulenger. Vol. I. 8vo. 1885.
Catalogue of Fossil Mammalia. Part 1. By R. Lydekker. 8vo. 1885.
Guide to the Mineral Gallery. 8vo. 1884.
Guide to the Galleries of Mammalia. 8vo. 1885.
Guide to the Galleries of Geology and Palæontology. 8vo. 1884.
Guide to the Collection of Fossil Fishes. 8vo. 1885.

Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Rendiconti. Vol. I. Fasc. 5, 6. 4to. 1885.
Astronomical Society, Royal—Monthly Notices, Vol. XLV. No. 4. 8vo. 1885.
Bankers, Institute of—Journal, Vol. VI Part 3. 8vo. 1885.
Bavarian Academy of Sciences, Royal—Sitzungsberichte, 1883, Heft 3; 1884, Heft 1, 2, and 3. 8vo. 1884
British Architects, Royal Institute of—Proceedings, 1884–5, Nos. 10, 11. 4to.
Chemical Society—Journal for March, 1885. 8vo.
Civil Engineers' Institution—Name-Index to Minutes of Proceedings, Vols. I.-LVIII. (1837-1879). 8vo. 1885.
Dax: Société de Borda—Bulletins, 2ᵉ Serie Dixième Année: Trimestre 1. 8vo. 1885.
East India Association—Journal, Vol. XVII. No. 3. 8vo. 1885.
Editors—American Journal of Science for March, 1885. 8vo.
Analyst for March, 1885. 8vo.
Athenæum for March, 1885. 4to.
Chemical News for March, 1885. 4to.
Engineer for March, 1885. fol.
Horological Journal for March, 1885. 8vo.
Iron for March, 1885. 4to.
Nature for March, 1885. 4to.
Revue Scientifique and Revue Politique et Littéraire for March, 1885. 4to.
Science Monthly, Illustrated, for March, 1885. 8vo.
Telegraphic Journal for March, 1885. 8vo.
Franklin Institute—Journal, No. 711. 8vo. 1885.
Geddes, Patrick, Esq. (the Author)—An Analysis of the Principles of Economics. Part 1. 8vo. 1885.
Geneva: Société de Physique et d'Histoire Naturelle—Mémoires, Tome XXVIII. Partie 2. 4to. 1884.
Geographical Society, Royal—Proceedings, New Series, Vol. VII. No. 3. 8vo. 1885.
Geological Institute, Imperial, Vienna—Jahrbuch: Band XXXIV. Heft 4. 8vo. 1884.
Verhandlungen, 1884, Nos. 1-18. 8vo. 1884.
Hanks, Henry G. Esq. (State Mineralogist)—California State Mining Bureau, Annual Report. 8vo. 1884.
Johns Hopkins University—University Circular, Nos. 37, 38. 4to. 1885.
American Journal of Philology, No. 20. 8vo. 1884.
Kerslake, Thomas, Esq. (the Author)—The Liberty of Independent Historical Research. 8vo. 1885.
Manchester Steam Users' Association—Boiler Explosions Act, 1882. Board of Trade Reports, Nos. 46 to 86. fol. 1884.
Mechanical Engineers' Institution—Proceedings, No. 1. 8vo. 1885.
Medical and Chirurgical Society, Royal—Proceedings, Vol. I. No. 7. 8vo. 1885.
Meteorological Office—Report of Meteorological Council, R. S. to 31 March, 1884. 8vo. 1885.
Miller, W. J. C. Esq. (the Registrar)—The Medical Register. 8vo. 1885.
The Dentist's Register. 8vo. 1885.
Numismatic Society—Chronicle and Journal, 1884, Part 4. 8vo.
Pharmaceutical Society of Great Britain—Journal, March, 1885. 8vo.
Royal Society of London—Proceedings, No. 235. 8vo. 1884-5.
Society of Arts—Journal, March, 1885. 8vo.
Society for Psychical Research—Proceedings, Part 7. 8vo. 1884.
Telegraph Engineers, Society of—Journal, Vol. XIV. No. 55. 8vo. 1885.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1885: Heft 2. 4to.
Vernon-Harcourt, Leveson Francis, Esq. M.A. (the Author)—Harbours and Docks. 2 vols. 8vo. 1885.

WEEKLY EVENING MEETING,

Friday, April 17, 1885.

SIR WILLIAM BOWMAN, Bart. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

PROFESSOR S. P. LANGLEY.

Sunlight and the Earth's Atmosphere.

THERE is, we may remember, a passage in which Plato inquires what would be the thoughts of a man who, having lived from infancy under the roof of a cavern, where the light outside was inferred only by its shadows, was brought for the first time into the full splendours of the sun.

We may have enjoyed the metaphor without thinking that it has any physical application to ourselves who appear to have no roof over our heads, and to see the sun's face daily; while the fact is that if we do not see that we have a roof over our heads in our atmosphere, and do not think of it as one, it is because it seems so transparent and colourless.

Now, I wish to ask your attention to-night to considerations in some degree novel, which appear to me to show that it is not transparent as it appears, and that this seeming colourlessness is a sort of delusion of our senses, owing to which we have never in all our lives seen the true colour of the sun, which is in reality blue rather than white, as it looks, so that this air all about and above us is acting like a coloured glass roof over our heads, or a sort of optical sieve, holding back the excess of blue in the original sunlight, and letting only the white sift down to us.

I will first ask you, then, to consider that this seeming colourlessness of the air may be a delusion of our senses, due to habit, which has never given us anything else to compare it with.

If that cave had been lit by sunshine coming through a reddish glass in its roof, would the perpetual dweller in it ever have had an idea but that the sun was red? How is he to know that the glass is "coloured" if he has never in his life anything to compare it with? How can he have any idea but that this is the sum of all the sun's radiations (corresponding to our idea of white or colourless light); will not the habit of his life confirm him in the idea that the sun is red; and will he not think that there is no colour in the glass so long as he cannot go outside to see? Has this any suggestion for us, who have none of us ever been outside our crystal roof to see?

We must all acknowledge in the abstract, that habit is equally strong in us whether we dwell in a cave or under the sky, that what we have thought from infancy will probably appear the sole possible explanation, and that, if we want to break its chain, we should put ourselves, at least in imagination, under conditions where it no longer binds us.

The 'Challenger' has dredged from the bottom of the ocean fishes which live habitually at great depths, and whose enormous eyes tell of the correspondingly faint light which must have descended to them through the seemingly transparent water. It will not be as futile a speculation as it may at first seem, to put ourselves in imagination in the condition of creatures under the sea, and ask what the sun may appear to be to them; for if the fish who had never risen above the ocean floor were an intelligent being, might *he* not plausibly reason that the dim greenish light of his heaven—which is all he has ever known—was the full splendour of the sun, shining through a medium which all his experience shows is transparent?

We ourselves are, in very fact, living at the floor of a great aërial sea, whose billows roll hundreds of miles above our heads. Is it not at any rate conceivable that we may have been led into a like fallacy from judging only by what we see at the bottom? May we not, that is, have been led into the fallacy of assuming that the intervening medium above us is colourless because the light which comes through it is so?

I freely admit that all men, educated or ignorant, appear to have the evidence of their senses that the air is colourless, and that pure sunlight is white, so that if I venture to ask you to listen to considerations which have lately been brought forward to show that it is the sun which is blue, and the air really acts like an orange veil or like a sieve which picks out the blue and leaves the white, I do so in the confidence that I may appeal to you on other grounds than those I could submit to the primitive man who has his senses alone to trust to; for the educated intelligence possesses those senses equally, and in addition the ability to interpret them by the light of reason, and before this audience it is to that interpretation that I address myself.

Permit me a material illustration. You see through this glass, which may typify the intervening medium of air or water, a circle of white light, which may represent the enfeebled disk of the sun when so viewed. Is this intervening glass coloured or not? It seems nearly colourless; but have we any right to conclude that it is so because it seems so? Are we not *taking it for granted* that the original light which we see through it is white, and that the glass is colourless, because the light seems unaltered, and is not an appeal to be made here from sense to reason, which, in the educated observer, recalls that white light is made of various colours, and that whether the original light is really white and the glass transparent, or the glass really coloured and so *making* the white, is to be decided only by experiment, by taking away the possibly deceptive medium? I can take away this glass, which was not colourless, but of a deep orange, and you see that the original light was not white, but intensely blue. If we could take the atmosphere away between us and the sun, how can we say that the same result might not follow? To make the meaning of our illustration clearer, observe that this blueness is not a pure spectral blue. It has in it red, yellow, blue, and all the colours which make up white, but blue in superabundance; so that, though the white is, so to say, latent there, the dominant effect is blue. The glass coloured veil does not put anything *in*, but acts I repeat like a sieve straining *out* the blue, and letting through to us the white light which was there in the bluishness, and so may not our air do so too?

I think we already begin to see that it is at any rate conceivable that we *may* have been hitherto under a delusion about the true colour of the sun, though of course this is not proving that we have been so, and it will at any rate, I hope, be evident that here is a question raised which ought to be settled, for the blueness of the sun, if proven, evidently affects our present knowledge in many ways, and will modify our present views in optics, in meteorology, and in numerous other things. In optics, because we should find that white light is *not* the sum of the sun's radiations, but only of those dregs of them which have filtered down to us; in meteorology because it is suggested that the temperature of the globe and the condition of man on it, depend in part on a curious selective action of our air, which picks out parts of the solar heat (for instance, that connected with its blue light), and holds them back, letting other selected portions come to us, and so altering the conditions on which this heat by which we live, depends; in other ways, innumerable, because, as we know, the sun's heat and light are facts of such central importance, that they affect almost every part of scientific knowledge.

It may be asked what suggested the idea that the sun may be blue rather than any other colour.

My own attention was first directed this way many years ago when measuring the heat and light from different parts of the sun's disk. It is known that the sun has an atmosphere of its own which tempers its heat, and, by cutting off certain radiations and not others, produces the spectral lines we are all familiar with. These lines we customarily study in connection with the absorbing vapours of sodium, iron, and so forth, which produce them; but my own attention was particularly given to the regions of absorption, or to the colour it caused, and I found that the sun's body must be deeply bluish, and that it would shed blue light except for this apparently colourless solar atmosphere, which really plays the part of a reddish veil, letting a little of the blue appear on the centre of the sun's disk where it is thinnest, and staining the edge red, so that to delicate tests the centre of the sun is a pale aqua-marine, and its edge a garnet. The effect I found to be so important, that if this all but invisible solar atmosphere were diminished by but a third part, the temperature of the British islands would rise above that of the torrid zone, and this directed my attention to the great practical importance of studying the action of our own terrestrial atmosphere on the sun, and the antecedent probability that our own air was also and independently making the really blue sun into an apparently white one. We actually know then, beyond conjecture, by a comparison of the sun's atmosphere, where it is thickest, and where it is thinnest, that an apparently colourless atmosphere *can* have such an effect, and analogous observations which I have carried on for many years, but do not now detail, show that the atmosphere of our own planet, this seemingly clear air in which we exist like creatures at the bottom of the sea, does do so.

We look up through our own air as through something so limpid in its purity that it appears scarcely matter at all, and we are apt to forget the enormous mass of what seems of such lightness, but which really presses with nearly a ton to each square foot, so that the weight of all the buildings in this great city, for instance, is less than that of the air above them.

I hope to shortly describe the method of proof that it too has been acting like an optical sieve, holding back the blue; but it may naturally be asked, "Can our senses have so entirely deceived us that they give no hint of this truth, if it be one? is the appeal wholly to recondite scientific methods, and are there no indications, at least, which we may gather for ourselves?" I think there are, even to our unaided eyes, indications that the seemingly transparent air really acts as an orange medium, and keeps the blue light back in the upper sky.

If I hold this piece of glass before my eyes, it seems colourless and transparent, but it is proved not to be so by looking through it edgewise, when the light, by traversing a greater extent, brings out its true colour, which is yellow. Everyone knows this in every-day experience. We shall not get the colour of the ocean by looking at it in a wine-glass, but by gazing through a great depth of it; and so it is with the air. If we look directly up, we look through where it is thinnest; but if we look horizontally through it towards the horizon, through great thicknesses, as at sunrise or sunset, is it not true that this air, where we see its real colour most plainly, makes the sun look very plainly yellow or orange.

We not only see here, in humid English skies, the "orange sunset waning slow," but most of us in these days of travel can perfectly testify that the clearest heavens the earth affords, the rosy tint on the snows of Mont Blanc, forerunning the dawn, or the warm glow of the sun as he sets in Egyptian skies, show this most clearly—show that the atmosphere holds back the blue rays by preference, and lets the orange through.

If, next, we ask, "What has become of the blue that it has stopped?" does not that very blue of the midday sky relate the rest of the story —that blue which Prof. Tyndall has told us is due to the presence of innumerable fine particles in the air, which act selectively on the solar waves, diffusing the blue light towards us? I hope it will be understood that Prof. Tyndall is in no way responsible for my own inferences; but I think it is safe at least to say that the sky is not self-luminous, and that, since it can only be shining blue at the expense of the sun, all the light this sky sends us has been taken by our atmosphere away from the direct solar beam, which would grow both brighter and bluer if this were restored to it.

If all that has been said so far renders it possible that the sun may be blue, you will still have a right to say that "possibilities" and "maybes" are not evidence, and that no chain of mere hypotheses will draw truth out of her well. We are all of one mind here, and I desire next to call your attention to what I think is evidence.

Remembering that the case of our supposed dweller in the cave who could not get outside, or that of the inhabitants of the ocean-floor who cannot rise to the surface, is really like our own, over whose heads is a crystalline roof which no man from the beginning of time has ever got outside of, an upper sea to whose surface we have never risen; we recognise that if we could rise to the surface, leaving the medium whose effect is in dispute wholly beneath us, we should see the sun as it is, and get proof of an incontrovertible kind; and that, if we cannot entirely do this, we shall get nearest to proof under our real circumstances by going as high as we can in a balloon, or by ascending a very high mountain. The balloon will not do, because we have to use heavy apparatus requiring a solid foundation. The proof to which I ask your kind attention, then, is that derived from the actual ascent of a remarkable mountain by an expedition undertaken for that purpose, which carried a whole physical laboratory up to a point where nearly one-half the whole atmosphere lay below us. I wish to describe the difference we found in the sun's energy at the bottom of the mountain and at the top, and then the means we took to allow for the effect of that part of the earth's atmosphere still over our heads even here, so that we may be said to have virtually got outside it altogether.

Before we begin our ascent, let me explain more clearly what we are going to seek. We need not expect to find that the original sunlight is a pure monochromatic blue by any means, but that though its rays contain red, orange, blue, and all the other spectral colours, the blue, the violet and the allied tints were originally there in disproportionate amounts, so that, though all which make white were present from the first, the refrangible end of the spectrum had such an excess of colour that the dominant effect was that of a bluish sun. In the same way, when I say briefly that our atmosphere has absorbed this excess of blue and let the white reach us, I mean, more strictly speaking, that this atmosphere has absorbed *all* the colours, but, selectively, taking out more orange than red, more green than orange, more blue than green; so that its action is wholly a taking *out*—an action like that which you now see going on with this sieve, sifting a mixture of blue and white beads, and holding back the blue while letting the white fall down.

This experiment only rudely typifies the action of the atmosphere, which is discriminating and selective in an amazing degree, and as there are really an infinite number of shades of colour in the spectrum, it would take for ever to describe the action in detail. It is merely for brevity, then, that we now unite the more refrangible colours under the general word "blue," and the others under the corresponding terms "orange" or "red."

All that I have the honour to lay before you, is less an announcement of absolute novelty than an appeal to your already acquired knowledge and to your reason as superior to the delusions of sense. I have, then, no novel experiment to offer, but to ask you to look at some familiar ones in a new light.

We are most of us familiar, for instance, with that devised by Sir Isaac Newton to show that white light is compounded of blue, red, and other colours, where, by turning a coloured wheel rapidly, all blend into a greyish white. Here you see the "seven colours" on the screen; but, though all are here, I have intentionally arranged them, so that there is too much blue, and the combined result is a very bluish white which may roughly stand for that of the original sun-ray. I now alter the proportion of the colours so as to virtually take out the excess of blue, and the result is colourless or white light. White, then, is not necessarily made by combining the "seven colours," or any number of them, unless they are there in just proportion (which is in effect what Newton himself says); and white, then, may be made out of such a bluish light as we have described, not by putting anything to it, but by taking away the excess which is there already.

Here, again, are two sectors—one blue, one orange-yellow with the blue in excess, making a bluish disk where they are revolved. I take out the excess of blue, and now what remains is white.

Here is the spectrum itself on the screen, but a spectrum which has been artificially modified so that the blue end is relatively too strong. I recombine the colours (by Prof. Rood's ingenious device of an elastic mirror), and they do not make a pure white, but one tinted with blue. I take out the original excess of blue, and what remains combines into a pure white. Please bear in mind that when we "put in" blue here, we have to do so by straining out other light through some obscuring medium, which makes the spectrum darker; but that, in the case of the actual sunlight, introducing more blue, introduces more light, and makes the spectrum brighter.

The spectrum on the screen ought to be made still brighter in the blue than it is—far, far brighter—and then it might represent to us the original solar spectrum before it has suffered any absorption either in the sun's atmosphere or our own. The Fraunhofer lines do not appear in it, for these, when found in the solar spectrum, show that certain individual rays have been stopped, or selected for absorption by the intervening atmospheres; and though even the few yards of atmosphere between the lamp and the screen absorb, it is not enough to show.

Our spectrum, as it appears before absorption, might be compared to an army divided into numerous brigades, each wearing a distinct uniform, one red, one green, one blue, so that all the colours are represented each by its own body. If, to represent the light absorbed as it progresses, we supposed that the army advances under a fire which thins its numbers, we should have to consider that (to give the case of nature) this destructive fire was directed chiefly against those divisions which were dressed in blue, or allied colours, so that the army was thinned out unequally, many men in blue being killed off for one in red, and that by the time it has advanced a certain distance under fire the proportion of the men in each brigade has been altered, the red being comparatively unhurt. Almost all absorption is thus selective in its action, and often in an astonishing degree, killing off, so to speak, certain rays in preference to others, as though by an intelligent choice, and destroying most, not only of certain divisions (to continue our illustration), but even picking out certain files in each company. Every ray, then, has its own individuality, and on this I cannot too strongly insist; for just as two men retain their personalities under the same red uniform, and one may fall and the other survive, though they touch shoulders in the ranks, so in the spectrum certain parts will be blotted out by absorption, while others next to them may escape.

To illustrate this selective absorption, I put a piece of didymium glass in the path of the ray. It will, of course, absorb some of the light, but instead of dimming the whole spectrum, we might almost say it has arbitrarily chosen to select one narrow part for action, in this particular case choosing a narrow file near the orange, and letting all the rest go unharmed. In this arbitrary way our atmosphere operates, but in a far more complex manner, taking out a narrow file here and another there, in hundreds of places, all through the spectrum, but on the whole much the most in the blue, the Frauenhofer lines being merely part of the evidence of this wonderful quasi-intelligent action which bears the name of selective absorption.

Before we leave this spectrum, let us recall one most important matter. We know that here beyond the red is solar energy in the form of heat which we cannot see, but not on that account any less important. More than half the whole power of the sun is here invisible, and if we are to study completely the action of our atmosphere, we shall have to pay great attention to this part, and find out some way of determining the loss in it, which will be difficult, for the ultra-red end is not only invisible, but compressed, the red end being shut up like the closed pages of a book, as you may notice by comparing the narrowness of the red with the width of the blue.

Now refraction by a prism is not the only way of forming a spectrum. Nature furnishes us colour not only from the rainbow, but from non-transparent substances like mother-of-pearl, where the iridescent hues are due to microscopically fine lines. Art has lately surpassed nature in these wonderful "gratings," consisting of pieces of polished metal, in which we see at first nothing to account for the splendid play of colour apparently pouring out from them like light from an opal, but which, on examination with a powerful microscope, show lines so narrow that there are from 50 to 100 in the thickness of a fine human hair, and all spaced with wonderful precision.

This grating is equal in defining power to many such prisms as we have just been looking at, but its light does not show well upon the screen. You will see, however, that its spectrum differs from that of the prism, in that in this case the red end is expanded, as compared with the violet, and the invisible ultra-red is expanded still more, so that this will be the best means for us to use in exploring that "dark continent" of invisible heat found not only in the spectrum of the sun, but of the electric light, and of all incandescent bodies, and of whose existence we already know from Herschel and Tyndall.

Now we cannot reproduce the actual solar spectrum on the screen without the sun itself, but here are photographs of it, which show parts of the losses the different colours have suffered on their way to us. We have before us the well-known Frauenhofer lines, due, you remember, not only to absorption in the sun's atmosphere, but also to absorption in our own. We have been used to think of them in connection with their cause, one being due to the absorption of iron-vapour in the sun, another to that of water-vapour in our own air, and so forth; but now I ask you to think of them only in connection with the fact that each is due to the absorption of some part of the original *light*, and that collectively they tell much of the story of what has happened to that light on its way down to us. Observe, for instance, how much thicker they lie in the blue end than in the red—another evidence of the great proportionate loss in the blue.

If we could restore all the lost light in these lines, we should get back partly to the original condition of things at the very fount, and, so far as our own air is concerned, that is what we are to ascend the mountain for—to see, by going up through nearly half of the atmosphere, what the rate of loss is in each ray by actual trial; then, knowing this rate, to be able to allow for the loss in the other part still above the mountain-top, and, finally, by recombining these rays to get the loss as a whole. Remember, however, always, that the most important part of the solar energy is in the dark spectrum which we do not see, but which, if we could see, we should probably find to have numerous absorption-spaces in it corresponding to the Frauenhofer lines, but where heat has been stopped out rather than light. To make our research thorough, then, we ought not to trust to the eye only, or even chiefly, but have some way of investigating the whole spectrum; the invisible in which the sun's power chiefly lies, as well as the visible, and both with an instrument that would discriminate the energy in these very narrow spaces, like an eye to see in the dark; and if science possesses no such instrument, then it may be necessary to invent one.

The linear thermopile is nearest to it of any, and we all here know what good work it has done, but even that is not sensitive enough to measure in the grating spectrum, in some parts of which the heat is four hundred times weaker than in that of a prism, and we want to observe this invisible heat in very narrow spaces. Something like this has been provided since by Captain Abney's most valuable researches, but these did not at the time go low enough for my purpose, and I spent nearly a year before ascending the mountain in inventing and perfecting the new instrument for measuring these, which I have called the "bolometer" or "ray-measurer." The principle on which it is founded is the same as that employed by my late friend, Sir Wm. Siemens, for measuring temperatures at the bottom of the sea, which is that a smaller electric current flows through a warm wire than through a cold one.

One great difficulty was to make the conducting wire very thin, and yet continuous, and for this purpose almost endless experiments were made, among other substances pure gold having been obtained by chemical means in a plate so thin that it transmitted a sea-green light through the solid substance of the metal. This proving unsuitable, I learned that iron had been rolled of extraordinary thinness in a contest of skill between some English and American ironmasters, and, procuring some, I found that 15,000 of the iron plates they had rolled, laid one on the other, would make but one English inch. Here is some of it, rolled between the same rolls which turn out plates for an iron-clad, but so thin that, as I let it drop, the iron plate flutters down like a dead leaf. Out of this the first bolometers were made, and I may mention that the cost of these earlier experiments was met from a legacy by the founder of the Royal Institution, Count Rumford. The iron is now replaced by platinum, in wires or rasher tapes from 1-2000 to 1-20,000th of an inch thick, one of which is within this button, where it is all but invisible, being far finer than a human hair. I will project it on the screen, placing a common small pin beside it as a standard of comparison. This button is placed in this ebonite case, and the thread is moved by this micrometer screw, by which it can be set like the spider line of a reticule; but by means of this cable, connecting it to the galvanometer, this thread acts as though sensitive, like a nerve laid bare to every indication of heat and cold. It is then a sort of sentient thing: what the eye sees as light it feels as heat, and what the eye sees as a narrow band of darkness (the Frauenhofer line) this feels as a narrow belt of cold, so that when moved parallel to itself and the Frauenhofer lines down the spectrum it registers their presence.

It is true we can see these in the visible spectrum, but you remember we propose to explore the invisible also, and since to this the dark is the same as the light, it will feel absorption lines in the infra-red which might remain otherwise unknown.

I have spent a long time in these preliminary researches; in indirect methods for determining the absorption of our atmosphere, and in experiments and calculations which I do not detail, but it is so often supposed that scientific investigation is a sort of happy guessing, and so little is realised of the labour of preparation and proof, that I have been somewhat particular in describing the essential parts of the apparatus finally employed, and now we must pass to the scene of their use.

We have been compared to creatures living at the bottom of the sea, who frame their deceptive traditional notions of what the sun is like from the feeble changed rays which sift down to them. Though such creatures could not rise to the surface, they might swim up towards it; and if these rays grew hotter, brighter, and bluer as they ascended, it would be almost within the capacity of a fish's mind to guess that they are still brighter and bluer at the top.

Since we children of the earth, while dwelling on it, are always at the bottom of a sea, though of another sort, the most direct method of proof I spoke of, is merely to group as far as we can and observe what happens, though as we are men, and not fishes, something more may fairly be expected of our intelligence than of theirs.

We will not only guess, but measure and reason, and in particular we will first, while still at the bottom of the mountain, draw the light and heat out into a spectrum, and analyse every part of it by some method that will enable us to explore the invisible as well as record the visible. Then we will ascend many miles into the air, meeting the rays on the way down, before the sifting process has done its whole work, and there analyse the light all over again, so as to be able to learn the different proportions in which the different rays have been absorbed, and by studying the action on each separate ray, to prove the state of things which must have existed before this sifting —this selective absorption—began.

It may seem at first that we cannot ascend far enough to do much good, since the surface of our aërial ocean is hundreds of miles overhead; but we must remember that the air grows thinner as we ascend, the lower atmosphere being so much denser, that about one-half the whole substance or mass of it lies within the first four miles, which is a less height than the tops of some mountains. Every high mountain, however, will not do, for ours must not only be very high but very steep, so that the station we choose at the bottom may be almost under the station we are afterwards to occupy at the top.

Besides, we are not going to climb a lofty, lonely summit like tourists to spend an hour, but to spend weeks; so that we must have fire and shelter, and above all we must have dry air to get clear skies. First I thought of the Peak of Teneriffe, but afterwards some point in the territories of the United States seemed preferable, particularly as the Government offered to give the Expedition, through the Signal Service, and under the direction of its head, General Hazen, material help in transportation and a military escort, if needed, anywhere in its own dominions. No summit in the eastern part of the United States rises much over 7000 feet; and though the great Rocky Mountains reach double this, their tops are the home of fog and mist, so that the desired conditions, if met at all, could only be found on the other side of the continent in Southern California, where the summits of the Sierra Nevadas rise precipitously out of the dry air of the great wastes in lonely peaks, which look eastward down from a height of nearly 15,000 feet upon the desert lands.

This remote region was, at the time I speak of, almost unexplored, and its highest peak, Mount Whitney, had been but once or twice ascended, but was represented to be all we desired could we once climb it. As there was great doubt whether our apparatus, weighing several thousand pounds, could possibly be taken to the top, and we had to travel 3000 miles even to get where the chief difficulties would begin, and make a desert journey of 150 miles after leaving the cars, it may be asked why we committed ourselves to such an immense journey to face such unknown risks of failure. The answer must be that mountains of easy ascent and 15,000 feet high are not to be found at our doors, and that these risks were involved in the nature of our novel experiment, so that we started out from no love of mere adventure, but from necessity, much into the unknown. The liberality of a citizen of Pittsburgh, to whose encouragement the enterprise was due, had furnished the costly and delicate apparatus for the expedition, and that of the trans-continental railroads, enabled us to take this precious freight along in a private car, which carried a kitchen, a steward, a cook, and an ample larder besides.

In this we crossed the entire continent from ocean to ocean, stopped at San Francisco for the military escort, went 300 miles south so as to get below the mountains, and then turned eastward again on to the desert, with the Sierras to the north of us, after a journey which would have been unalloyed pleasure except for the anticipation of what was coming as soon as we left our car. I do not indeed know that one feels the triumphs of civilisation over the opposing forces of Nature anywhere more than by the sharp contrasts which the marvellous luxury of recent railroad accommodation gives to the life of the desert. When one is in the centre of one of the great barren regions of the globe, and, after looking out from the windows of the flying train on its scorched wastes for lonely leagues of habitless desolation, turns to his well-furnished dinner-table, and the fruit and ices of his dessert, he need not envy the heroes of Oriental story, who were carried across dreadful solitudes in a single night on the backs of flying genii. Ours brought us over 3000 miles to the Mojave Desert. It was growing hotter and hotter when the train stopped in the midst of vast sandwastes a little after midnight. Roused from our sleep, we stepped on to the brown sand, and saw our luxurious car roll away in the distance, experiencing a transition from the conditions of civilisation to those almost of barbarism, as sharp as could well be imagined. We commenced our slow toil northward with a thermometer at 110° in the shade, if any shade there be in the shadeless desert, which seemed to be chiefly inhabited by rattlesnakes of an ashen-grey colour, and a peculiarly venomous bite. There is no water save at the rarest intervals, and the soil at a distance seems as though strewed with sheets of salt, which aids the delusive show of the mirage. These are, in fact, the ancient beds of dried-up salt lakes or dead seas, some of them being below the level of the ocean; and such a one on our right, though only about twenty miles wide, has earned the name of "Death Valley," from the number of human beings who have perished in it. Formerly an emigrant train, when emigrants crossed the continent in caravans, had passed through the great Arizona deserts in safety until, after their half-year's journey, their eyes were gladdened by the snowy peaks of the Sierras looking delusively near. The goal of their long toil seemed before them; only this one more valley lay between, and into this they descended, thinking to cross it in a day—but they never crossed it. Afterwards the long line of wagons was found with the skeletons of the animals in the harness, and by them those of men, women, and little children dead of thirst, and some relics of the tragedy remained at the time of our journey. I cite this as an indirect evidence of the phenomenal dryness of the region—a dryness which, so far, served our object, which was, in part, to get rid as much as possible of that water vapour which is so well known to be a powerful absorber of the solar heat.

Everything has an end, and so had that journey, which finally brought us to the goal of our long travel, at the foot of the highest peak of the Sierras, Mount Whitney, which rose above us in tremendous precipices, that looked hopelessly insurmountable and wonderfully near. The whole savage mountain region in its slow rises from the west, and its descent to the desert plains in the east, is more like the chain called the Apennines, in the moon, than anything I know on the earth. The summits are jagged peaks like Alpine "needles," looking in the thin air so delusively near, that, coming on such a scene unprepared, one would almost say they were large grey stones a few fields off, with an occasional little white patch on the top, that might be a handkerchief or a sheet of paper dropped there. But the telescope showed that the seeming stones were of the height of many Snowdons piled on one another, and the white patches occasional snow-fields, looking how invitingly cool, from the torrid heat of the desert, where we were encamped by a little rivulet that ran down from some unseen ice-lake in that upper air. Here we pitched our tents and fell to work (for you remember we must have two stations, a low and a high one, to compare the results), and here we laboured three weeks in almost intolerable heat, the instruments having to be constantly swept clear of the red desert dust which the hot wind brought. Close by these tents a thermometer covered by a single sheet of glass, and surrounded by wool, rose to 237° in the sun, and sometimes in the tent, which was darkened for the study of separate rays, the heat was absolutely beyond human endurance. Finally, our apparatus was taken apart and packed in small pieces on the backs of mules, who were to carry it by a ten days' journey through the mountains to the other side of the rocky wall which, though only ten or twelve miles distant, arose miles above our heads; and, leaving these mule trains to go with the escort by this longer route, I started with a guide by a nearer way to those white gleams in the upper skies, that had daily tantalised us below in the desert with suggestions of delicious, unattainable cold. That desert sun had tanned our faces to a leather-like brown, and the change to the cooler air as we ascended was at first delightful. At an altitude of 5000 feet we came to a wretched band of nearly naked savages, crouched around their camp fire, and at 6000 found the first scattered trees; and here the feeble suggestion of a path stopped, and we descended a ravine to the bed of a mountain stream, up which we forced our way, cutting through the fallen trees with an axe, fighting for every foot of advance, and finally passing what seemed impassable. It was interesting to speculate as to the fate of our siderostat mirrors and other precious freight, now somewhere on a similar road, but quite useless. We were committed now, and had to make the best of it—and, besides, I had begun to have my attention directed to a more personal subject. This was, that the colder it grew the more the sun burnt the skin—quite literally burnt, I may say, so that by the end of the third day my face and hands, case-hardened, as I thought, in the desert, began to look as if they had been seared with red-hot irons, here in the cold where the thermometer had fallen to freezing at night; and still as we ascended the paradoxical effect increased; the colder it grew about us, the hotter the sun blazed above.

We have all heard probably of this curious effect of burning in the midst of cold, and some of us may have experienced it in the Alps, where it may be aided by reflection from the snow, which we did not have about us at any time except in scattered patches, but here by the end of the fourth day my face was scarcely recognisable, and it almost seemed as though sunbeams up here were different things, and contained something which the air filters out before they reach us in our customary abodes. Radiation here is increased by the absence of water vapour too, and on the whole this intimate personal experience fell in almost too well with our anticipations that the air is an even more elaborate trap to catch the sunbeams than had been surmised, and that this effect of selective absorption and radiation was intimately connected with that change of the primal energies and primal colour of the sun which we had climbed towards it to study.

On the fourth day, after break-neck ascents and descents, we finally ascended by a ravine, down which leaped a cataract, till, at nightfall, we reached our upper camp, which was pitched by a little lake, one of the sources of the waterfall, at a height of about 12,000 feet, but where we seemed in the bottom of a valley, nearly surrounded as we were by an amphitheatre of rocky walls which rose perpendicularly to the height of Gibraltar from the sea, and cut off all view of the desert below or even of the peak above us.

The air was wonderfully clear, so that the sun set in a yellow rather than an orange sky, which was reflected in the little ice-rimmed lakes and from occasional snow-fields on the distant waste of lonely mountain summits on the west.

The mule train sent off before by another route, had not arrived when we got to the mountain camp, and we realised that we were far from the appliances of civilisation by our inability to learn about our chief apparatus, for here, without post or telegraph, we were as completely cut off from all knowledge of what might be going on with it in the next mountain ravine as a ship at sea is of the fate of a vessel that sailed before from the same port. During the enforced idleness we ascended the peak nearly 3000 feet above us, with our lighter apparatus, leaving the question of the ultimate use of the heavy ones to be settled later. There seemed little prospect of carrying it up, as we climbed where the granite walls had been split by the earthquakes, letting a stream of great rocks, like a stone river, flow down through the interstices by which we ascended, and, in fact, the heavier apparatus was not carried above the mountain camp.

The view from the very summit was over numberless peaks on the west to an horizon fifty miles away, of unknown mountain-tops, for, with the exception of the vast ridge of Mount Tyndall, and one or two less conspicuous ones, these summits are not known to fame, and, wonderful as the view may be, all the charm of association with human interest which we find in the mountain landscape of older lands is here lacking.

It was impossible not to be impressed with the savage solitude of this desert of the upper air, and our remoteness from man and his works, but I turned to the study of the special things connected with my mission. Down far below the air seemed filled with reddish dust that looked like an ocean. This dust is really present everywhere (I have found it in the clear air of Etna), and though we do not realise its presence in looking up through it, to one who looks down on it, the dwellers on the earth seem indeed like creatures at the bottom of a troubled ocean. We had certainly risen towards the surface, for about us the air was of exquisite purity, and above us the sky was of such a deep violet blue, as I have never seen in Egypt or Sicily, and yet even this was not absolutely pure, for separately invisible, the existence of fine particles could yet be inferred from their action on the light near the sun's edge, so that even here we had not got absolutely above that dust shell which seems to encircle our whole planet. But we certainly felt ourselves not only in an upper, but a different region. We were on the ridge of the continent, and the winds which tore by had little in common with the air below, and were bearing past us (according to the geologists) dust which had once formed part of the soil of China, and been carried across the Pacific Ocean; for here we were lifted into the great encircling currents of the globe, and, "near to the sun in lonely lands," were in the right conditions to study the differences between his rays at

the surface and at the bottom of that turbid sea where we had left the rest of mankind. We descended the peak and hailed with joy the first arrival of our mule trains with the requisite apparatus at the mountain camp, and found that it had suffered less than might be expected, considering the pathless character of the wilderness. We went to work to build piers and mount telescopes and siderostats, in the scene shown by the next illustration on the screen, taken from a sketch of my own, where these rocks in the immediate foreground rise to thrice the height of St. Paul's. We suffered from cold (the ice forming 3 inches deep in the tents at night) and from mountain sickness, but we were too busy to pay much attention to bodily comfort, and worked with desperate energy to utilise the remaining autumn days, which were all too short.

Here, as below, the sunlight entered a darkened tent, and was spread into a spectrum, which was explored throughout by the bolometer, measuring, on the same separate rays which we had studied below in the desert, all of which were different up here, all having grown stronger, but in very different proportions. On the screen is the spectrum as seen in the desert, drawn on a conventional scale, neither prismatic nor normal, but such that the intensity of the energy shall be the same in each part, as it is represented here by these equal perpendiculars in every colour. Fix your attention on these three as types, and you will see better what we found on the mountain, and what we inferred as to the state of things still higher up, at the surface of the aëreal sea.

You will obtain, perhaps, a clearer idea, however, from the following statement, where I use, not the exact figures used in calculation, but round numbers, to illustrate the process employed. I may premise that the visible spectrum extends from H (in the extreme blue) to A (in the deepest red), or from near 40 (the ray of 40-100,000ths of a millimetre in wave-length) to near 80. All below 80, to the right, is the invisible infra-red spectrum.

Now, the shaded curve above the spectrum represents the amount of energy in the sun's rays at the foot of the mountain, and was obtained in this way:—Fix your attention for a moment on any single part of the spectrum, for instance, that whose wave-length is 60. If the heat in this ray, as represented by the bolometer at the foot of the mountain, was (let us suppose) 2°, on any arbitrary scale we draw a vertical line, 2 inches, or 2 feet high over that part of the spectrum. If the heat at another point, such as 40, were but a $\frac{1}{8}$°, a line would be drawn there a quarter of an inch high, and so on, till these vertical lines mark out the shaded parts of the drawing, the gaps and depressions in whose outline correspond to the "cold bands" already spoken of. Again, if on top of the mountain we measure all these over once more, we shall find all are hotter, so that we must up there make all our lines higher, but *in very different proportions*. At 60, for instance, the heat (and light) may have grown from 2° to 3°, or increased one-half, while above 40 the heat (and light) may have grown from $\frac{1}{5}$° to 1°, or increased five times. These mountain measurements give another spectrum, the energies in each part of which are defined by the middle dotted line, which we see indicates very much greater energy whether heat or light in the blue end than below. Next, the light or heat which would be observed at the surface of the atmosphere is found in this way. If the mountain top rises through one-half the absorbing mass of this terrestrial atmosphere (it does not quite do so, in fact), and by getting rid of that lower half, the ray 60 has grown in brightness from two to three, or half as much again; in going up to the top it would gain half as much more, or become 4½, while the ray near 40, which has already increased to five times what it was, would increase five times more, or to 25. Each separate ray increasing thus nearly in some geocentric progression (though the heat, as a whole, does not), you see how we are able, by repeating this process at every point, to build up our outer or highest curve, which represents the light and heat at the surface of the atmosphere. These have grown out of all proportion at the blue end, as you see by the outer dotted curve, and now we have attained, by actual measurement, that evidence which we sought, and by thus reproducing the spectrum outside the atmosphere, and then recombining the colours by like methods to those you have seen on the screen, we finally get the true colour of the sun, which tends, broadly speaking, to blue.

It is so seldom that the physical investigator meets any novel fact quite unawares, or finds anything except that in the field where he is seeking, that he must count it an unusual experience to come unexpectedly on even the smallest discovery. This experience I had on one of the last days of work on the spectrum on the mountain. I was engaged in exploring that great invisible heat region, still but so partially known, or, rather, I was mapping in that great "dark continent" of the spectrum, and by the aid of the exquisite sky and the new instrument (the bolometer) found I could carry the survey further than any had been before. I substituted the prism for the grating, and measured on in that unknown region till I had passed the Ultima Thule of previous travellers, and finally came to what seemed the very end of the invisible heat spectrum beyond what had previously been known. This was in itself a return for much trouble, and I was about rising from my task when it occurred to me to advance the bolometer still farther, and I shall not forget the surprise and emotion with which I found new and yet unrecognised regions below—a new invisible spectrum beyond the farthest limits of the old one.

I will anticipate here by saying that after we got down to lower earth again the explorations and mapping of this new region was continued. The amount of solar energy included in this new extension of the invisible region is much less than that of the visible spectrum, while its length upon the wave-length scale is equal to all that previously known, visible and invisible, as you will see better by this view, having the same thing on the normal as well as the prismatic scale. If it be asked which of these is correct, the answer is "both of them." Both rightly interpreted mean just the same thing, but in the lower one we can more conveniently compare the ground of the researches of others with these. These great gaps I was at first in doubt about, but more recent researches at Alleghany make it probable that they are caused by absorption in our own atmosphere, and not in that of the sun.

We would gladly have stayed longer, in spite of physical discomfort, but the formidable descent and the ensuing desert journey were before us, and certainly the reign of perpetual winter around us grew as hard to bear as the heats of the desert summer had been. On September 10 we sent our instruments and the escort back by the former route, and, ourselves unencumbered, started on the adventurous descent of the eastern precipices by a downward climb, which, if successful, would carry us to the plains in a single day. I at least shall never forget that day, nor the scenery of more than Alpine grandeur which we passed in our descent, after first climbing by frozen lakes in the northern shadow of the great peak, till we crossed the eastern ridges, through a door so narrow that only one could pass it at a time, by clinging with hands and feet as he swung round the shoulder of the rocks—to find that he had passed in a single minute from the view of winter to summer, the prospect of the snowy peaks behind shut out, and instantly exchanged for that below of the glowing valley and the little oasis where the tents of the lower camp were still pitched, the tents themselves invisible, but the oasis looking like a green scarf dropped on the broad floor of the desert. We climbed still downward by scenery unique in my recollection. This view of the ravine on the screen is little more than a memorandum made by one of the party in a few minutes' halt part-way down, as we followed the ice-stream between the tremendous walls of the defile which rose 2000 feet, and between which we still descended, till, toward night, the ice-brook had grown into a mountain torrent, and, looking up the long vista of our day's descent, we saw it terminated by the Peak of Whitney, once more lonely in the fading light of the upper sky.

This site, in some respects unequalled for a physical observatory, is likely, I am glad to say, to be utilised, the President of the United States having, on the proper representation of its value to science, ordered the reservation for such purposes of an area of 100 square miles about and inclusive of Mount Whitney.

There is little more to add about the journey back to civilisation, where we began to gather the results of our observation, and to reduce them—to smelt, so to speak, the metal from the ore we had brought home—a slow but necessary process, which has occupied a large part of two years.

The results stated in the broadest way mean that the sun is blue—but mean a great deal more than that; this blueness in itself being perhaps a curious fact only, but in what it implies, of practical moment.

We deduce in connection with it a new value of the solar heat, so far altering the old estimates that we now find it capable of melting a shell of ice sixty yards thick annually over the whole earth, or, what may seem more intelligible on its practical bearings, of exerting over one horse-power for each square yard of the normally exposed surface. We have studied the distribution of this heat in a spectrum whose limits on the normal scale our explorations have carried to an extent of rather more than twice what was previously known, and we have found that the total loss by absorption from atmosphere is nearly double what has been heretofore supposed.

We have found it probable that the human race owes its existence and preservation even more to the heat-storing action of the atmosphere than has been believed.

The direct determination of the effect of water vapour in this did not come within our scope; but that the importance of the blanketing action of our atmospheric constituents has been in no way overstated, may be inferred when I add that we have found by our experiments that if the planet were allowed to radiate freely into space without any protecting veil, its sunlit surface would probably fall, even in the tropics, below the temperature of freezing mercury.

I will not go on enumerating the results of these investigations, but they all flow from the fact, which they in turn confirm, that this apparently limpid sea above our head, and about us, is carrying on a wonderfully intricate work on the sunbeam, and on the heat returned from the soil, picking out selected parts in hundreds of places, sorting out incessantly at a task which would keep the sorting demons of Maxwell busy, and as one result, changing the sunbeam on its way down to us in the way we have seen.

I have alluded to the practical utilities of these researches, but practical or not, I hope we may feel that such facts as we have been considering about sunlight and the earth's atmosphere may be stones useful in the future edifice of science, and that if not in our own hands then in those of others, when our day is over, they may find the best justification for the trouble of their search, in the fact that they prove of some use to man.

May I add an expression of my personal gratification in the opportunity with which you have honoured me of bringing these researches before the Royal Institution, and of my thanks for the kindness with which you have associated yourselves for an hour, in retrospect at least, with that climb toward the stars which we have made together, to find, from light in its fulness, what unsuspected agencies are at work to produce for us the light of common day.

[S. P. L.]

WEEKLY EVENING MEETING,

Friday, April 24, 1885.

Sir William Bowman, Bart. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

William Carruthers, Esq. F.R.S.

British Fossil Cycads and their relation to Living Forms.

(Abstract deferred.)

ANNUAL MEETING.

Friday, May 1, 1885.

The Duke of Northumberland, D.C.L. LL.D. President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1884, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to above 85,400*l.*, entirely derived from the Contributions and Donations of the Members.

Forty-four new Members paid their Admission Fees in 1884.

Sixty-three Lectures and Twenty Evening Discourses were delivered in 1884.

The Books and Pamphlets presented in 1884 amounted to about 276 volumes, making, with 506 volumes (including Periodicals bound) purchased by the Managers, a total of 782 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, D.C.L. LL.D.
Treasurer—George Busk, Esq. F.R.S.
Secretary—Sir Frederick J. Bramwell, F.R.S.

Managers.

Sir Frederick Abel, C.B. D.C.L. F.R.S.
George Berkley, Esq. M.I.C.E.
Sir William Bowman, Bart. LL.D. F.R.S.
Joseph Brown, Esq. Q.C.
William Crookes, Esq. F.R.S.
Warren de la Rue, Esq. M.A. D.C.L. F.R.S.
Captain Douglas Galton, C.B. D.C.L. F.R.S.
The Hon. Sir Wm. Robt. Grove, M.A. D.C.L. LL.D. F.R.S.
Sir J. D. Hooker, K.C.S.I. C.B. M.D. D.C.L. LL.D. F.R.S.
William Huggins, Esq. D.C.L. LL.D. F.R.S.
Hugo W. Müller, Esq. Ph.D. F.R.S.
The Right Hon. Earl Percy, M.P.
Henry Pollock, Esq.
John Rae, M.D. LL.D. F.R.S.
The Right Hon. Lord Rayleigh, M.A. D.C.L. F.R.S.

Visitors.

The Lord Brabazon.
Stephen Busk, Esq.
Arthur Herbert Church, Esq. M.A.
Frank Crisp, Esq. LL.B. B.A. F.L.S.
Henry Herbert Stephen Croft, Esq. M.A.
Rear-Admiral Herbert P. De Kantzow, R.N.
William Henry Domville, Esq.
Alfred Gutteres Henriques, Esq. F.G.S.
Rev. John Macnaught, M.A.
Robert James Mann, M.D. F.R.C.S.
John W. Miers, Esq.
William Henry Preece, Esq. F.R.S.
Lachlan Mackintosh, Rate, Esq. M.A.
William Chandler Roberts, Esq. F.R.S.
Basil Woodd Smith, Esq. F.R.A.S.

WEEKLY EVENING MEETING,

Friday, May 1, 1885.

Sir William Bowman, Bart. LL.D. F.R.S. Manager and Vice-President, in the Chair.

The Right Hon. Lord Rayleigh, M.A. D.C.L. LL.D. F.R.S. *M.R.I.*

Water Jets and Water Drops.

(No Abstract.)

GENERAL MONTHLY MEETING,

Monday, May 4, 1885.

Sir William Bowman, Bart. LL.D. F.R.S. Manager and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:—

Sir William Bowman, Bart. LL.D. F.R.S.
Warren de la Rue, Esq. M.A. D.C.L. F.R.S.
The Hon. Sir William R. Grove, M.A. D.C.L. LL.D. F.R.S.
Sir Joseph D. Hooker, K.C.S.I. C.B. M.D. D.C.L. LL.D. F.R.S.
William Huggins, Esq. D.C.L. LL.D. F.R.S.
The Right Hon. Lord Rayleigh, M.A. D.C.L. F.R.S.
George Busk, Esq. F.R.S. Treasurer.
Sir Frederick J. Bramwell, F.R.S. Secretary.

His Royal Highness Prince Albert Victor Christian Edward, K.G.
His Royal Highness Prince George Frederick Ernest Albert, K.G.

were elected Honorary Members of the Royal Institution.

The Managers reported, that at their Meeting held this day the following Resolution was unanimously agreed to:—

"The Managers of the Royal Institution in accepting Sir William Bowman's resignation of the office of Honorary Secretary, desire to express to him their most cordial appreciation of the manner in which he accepted that post, and of the admirable way in which he has discharged its duties during the time for which he has been so good as to fill it.

"Sir William's own scientific distinctions, and the interest he has always taken in the prosperity of the Institution, pointed him out as one eminently fitted to occupy a foremost place among its officials. His extensive professional engagements alone seemed to present an objection to his undertaking the additional labours thus imposed upon him. This consideration was not allowed by Sir William to stand in the way; he generously assumed the further burden, and has acquitted himself in the execution of the important and multifarious functions of the honorary Secretaryship in a mode thoroughly worthy of the Institution and of himself—and has earned the lasting gratitude and thanks of the Managers which they now resolve should be tendered to him along with their deeply felt regret upon his retirement."

Montague Cookson, Esq. Q.C. D.C.L.
William Richard Minter Glasier, Esq.
Ernest Kümpers, Esq.
William Robertson, Esq.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

The Secretary of State for India—Report on Public Instruction in Bengal, 1883–4, fol. 1884.
Academy of Natural Sciences, Philadelphia—Proceedings, 1884, Part 3. 8vo. 1885.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. Vol. I. Fasc. 7, 8. 8vo. 1884–5.
Asiatic Society of Bengal—Proceedings, No. 11. 8vo. 1884.
Journal, Vol. LIII. Part I. 8vo. 1884.
Asiatic Society, Royal—Journal, Vol. XVII. Part 2. 8vo. 1885.
Astronomical Society, Royal—Monthly Notices, Vol. XLV. No. 5. 8vo. 1885.
Atheneo do Porto—Revista Scientifica, Nos. 1–3. 8vo. 1885.
Bankers, Institute of—Journal, Vol. VI. Part 4. 8vo. 1885.
Behnke, Emil, Esq. and Lennox Browne, Esq. F.R.C.S. (the Authors)—The Child's Voice. 12mo. 1885.
British Architects, Royal Institute of—Proceedings, 1884–5, No. 12. 4to.
British Museum (Natural History)—Catalogue of Birds. Vol. X. 8vo. 1885.
Guide to the Gallery of Reptilia. 8vo. 1885.
Browne, Lennox, Esq. F.R.C.S. (the Author)—Voice Use and Stimulants. 12mo. 1885.
Brymner, Douglas, Esq. (Archivist)—Report on Canadian Archives, 1884. 8vo. 1885.
Cambridge Philosophical Society—Proceedings, Vol. V. Parts 1–3. 8vo. 1884–5.
Transactions, Vol. XIV. Part I. 4to. 1885.
Carboni-Grio, Sigr. D. (the Author)—I Terremoti di Calabria e Sicilia. 8vo. Napoli, 1885.
Chemical Society—Journal for April, 1885. 8vo.
Chief Signal Officer, U.S. Army—Annual Report for 1883. 8vo. 1884.
Collins, Louis, Esq. (the Author)—The Advertisers' Guardian. 8vo. 1885.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. V. Part 2. 8vo. 1885.
Dawson, George M. Esq. F.G.S. (the Author)—Superficial Deposits and Glaciation of the District in the Vicinity of the Bow and Belly Rivers. 8vo. 1885.
Editors—American Journal of Science for April, 1885. 8vo.
Analyst for April, 1885. 8vo.
Athenæum for April, 1885. 4to.
Chemical News for April, 1885. 4to.
Engineer for April, 1885. fol.
Horological Journal for April, 1885. 8vo.
Iron for April, 1885. 4to.
Nature for April, 1885. 4to.
Science Monthly, Illustrated, for April, 1885.
Telegraphic Journal for April, 1885. 8vo.
Franklin Institute—Journal, No. 712. 8vo. 1885.
Geographical Society, Royal—Proceedings, New Series, Vol. VII. No. 4. 8vo. 1885.
Johns Hopkins University—Local Institutions of Virginia. By E. Ingle. 8vo. 1885.
Johnson, G. S. Esq. (the Author)—Elementary Nitrogen. 8vo. 1885.
Linnean Society—Journal, Nos. 107, 136. 8vo. 1885.
Lisbon, Sociedade de Geographia—Boletim: 4ᵉ Serie, Nos. 10, 11. 8vo. 1883.
Manchester Geological Society—Transactions, Vol. XVIII. Parts 4–7. 8vo. 1885.
Medical and Chirurgical Society, Royal—Proceedings, New Series, Nos. 8, 9, 8vo. 1885.
Meteorological Office—Monthly Weather Report, December 1884, January 1885. 4to.
Meteorological Society, Royal—Quarterly Journal, No. 53. 8vo. 1885.
Meteorological Record, No. 15. 8vo. 1885.

National Association for Social Science, Transactions, Birmingham, 1884. 8vo. 1885.

North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXIV. Part 2. 8vo. 1885.

Account of the Strata of Northumberland and Durham. F to K. 8vo. 1885.

Odontological Society of Great Britain—Transactions, Vol. XVII. No. 5. New Series. 8vo. 1885.

Pharmaceutical Society of Great Britain—Journal, April, 1885. 8vo.

Photographic Society—Journal, New Series, Vol. IX. Nos. 5, 6. 8vo. 1885.

Preussische Akademie der Wissenschaften, Sitzungsberichte, XL.–LIV. 8vo. 1884–5.

Royal Society of London—Philosophical Transactions, Vol. CLXXV. Part 2. 4to. 1885.

Society of Arts—Journal, April, 1885. 8vo.

St. Pétersbourg, Académie des Sciences—Memoirs, Tome XXXII. No. 13. 4to. 1884.

United States Geological Survey—Third Annual Report, 1881–2. 4to. 1883.

Geology of the Comstock Lode and the Washoe District. With Atlas. By G. F. Becker. 4to. 1882.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1885: Heft 3. 4to.

Victoria Institute—Journal No. 72. 8vo. 1885.

Wild, Dr. H. (*the Director*)—Annalen des Physikalischen Central-Observatoriums, 1882. 4to. 1883.

Zoological Society—Proceedings, 1884, Part 4. 8vo. 1885.

WEEKLY EVENING MEETING,

Friday, May 8, 1885.

Sir William Bowman, Bart. LL.D. F.R.S. Manager and Vice-President, in the Chair.

W. F. R. Weldon, Esq.

On Adaptation to Surroundings as a Factor in Animal Development.

(Abstract deferred.)

WEEKLY EVENING MEETING,

Friday, May 15, 1885.

Sir Frederick Abel, C.B. D.C.L. F.R.S. Manager, in the Chair.

Professor J. Burdon Sanderson, M.D. LL.D. F.R.S.

Cholera: its Cause and Prevention.

The interest excited in cholera by its presence in Europe during last summer and autumn was reawakened in the spring by the prospect of a war which might have brought us face to face with an enemy much more formidable than the armies of Russia. War is no longer in immediate prospect, so that for the present we need not think of cholera in connection with Asia Minor or the Black Sea. But the epidemic which is now raging with such pitiless fury in the Mediterranean provinces of Spain makes us all feel that the threat of 1884 may be fulfilled in 1885. There is probably no serious ground for apprehending that we shall have to do with cholera in England this year; the chance, however, is sufficiently near to make it reasonable to inquire whether any useful information as to the causes of cholera, or the way in which it can best be guarded against, has been gained since the last time that the disease visited our shores.

In dealing with cholera, as in other matters in respect of which conduct must be guided by knowledge of some kind, the question what sort of knowledge is best and most valuable comes prominently to the front, and is one on which those who profess to follow the scientific method, and those who profess to be guided by what they are pleased to call common sense, are apt to entertain different opinions. The question is in reality not between two kinds of knowledge, but between two ways of acquiring the same kind of knowledge. Those of us who have studied cholera at home in the hospital ward or in the laboratory approach the subject on one side. Those whose lives, like that of my friend Dr. J. M. Cunningham, the Sanitary Commissioner with the Government of India, have for the most part been spent in a prolonged encounter with cholera year after year, as it presents itself in prisons and armies and among the multitudinous populations of our Indian Empire, from another. But we are all seeking the same kind of knowledge, and what is more, we all tend to the same conclusions. If, for example, a comparison be made of the recent work published by Dr. Cunningham, "Cholera: What can the State do to prevent it?" in which he professes to confine himself to considerations of common sense and deprecates the interference of science with practical questions, with the lecture given a few months ago to the people of Munich by Professor von Pettenkofer, who is acknowledged to be one of the highest scientific authorities on the etiology of cholera, it will be found that the German *Gelehrter* and the English administrator say practically the same thing.

As this paper is intended for the perusal of persons who do not specially concern themselves with pathology, I will enter as little as possible upon subjects of controversy, regarding it as of much more importance that those notions as to the cause and nature of cholera, about which there is no dispute, should be generally understood, than that the claims of rival investigators should be vindicated. In the slow process by which new knowledge is acquired, strife is a necessary and unquestionably a productive element. Burning questions arise wherever and whenever scientific investigation bears, or appears to bear, on practical action. Eventually they find their solution; but in the meantime it is almost impossible for those who are immediately concerned in discussing them to guard against the influence of personal antagonisms and predilections. As regards all recent questions of this kind, I think that I am myself in a position to look at them from a distance, for I have had no direct concern with cholera since 1866. I will therefore ask the reader to regard me neither as a contagionist nor as a localist, and to dismiss the "comma-bacillus" from his mind until we have had time to take a general view of the tendencies which this great world plague has manifested in its dealings with mankind since it first found its way into Europe.

It is agreed by all authorities that cholera is native in India, and particularly in the district where it is now "endemic"—namely, in the district which corresponds roughly to the deltas of the Ganges and Brahmaputra and the district of Cuttack. As, however, it for the most part confined its ravages to the native populations, with whom at that time our relations were much less direct and intimate than they are now, it excited no general interest, and was indeed so little known to medical men that when in 1817 the disease broke out at Jessore, near Calcutta, it was believed to be an entirely new malady. Even now there are some writers who speak of Jessore as the "cradle of cholera" and the year 1817 as the starting-point of its history, notwithstanding that the enquiries which were then initiated showed not only that in Bengal the disease was an annual visitor, but that in Calcutta itself it was fatally prevalent in the native town several weeks before Dr. Tytler was called to see the first case at Jessore.

The great epidemic of 1817 and 1818 was distinguished from previous ones by its extent and destructiveness, but chiefly by the circumstance that in this year it became for the first time a serious obstacle to English conquest. How or when it began it is probably impossible to determine, for evidence exists of its presence in July 1817 within a few weeks at places so distant from one another as Patna and Dacca. Two months later it was at Benares, Allahabad, and Mirzapore; and in October of the same year an event occurred which at once gave the disease a significance it had not before possessed. The Marquis of Hastings, with an army of over 10,000 Europeans and a much larger native force, was in the Bundelcund, not far from Allahabad, where cholera was then raging. Cholera had on several previous occasions interfered with military operations, but this time it attacked Hastings' European troops with a violence of which there had before been no example. The pestilence continued for several weeks with unabated destructiveness, until early in November the army was withdrawn from the Bundelcund and moved westwards in its march towards Gwalior, on which the mortality at once subsided. Thousands of dead and dying were left behind, but cholera was left behind with them, and a lesson was learned which has since been often repeated in Indian experience—that when a military force is encountered by cholera, removal from the infected locality is the only effectual way of checking it.

In 1818 cholera overspread the whole Indian Peninsula. Westward it extended up the Ganges valley to Delhi and Agra, and eventually found its way across the Sutlej to Lahore. Southwards it flanked the line of the Vindhya, attacked Nagpore, and thence spread to other places in Central India. Along the east coast there were destructive epidemics at Vizagapatam, in the deltas of the Godavery and Kistnah, at Madras and Pondicherry, and various other places further south. In 1819 Ceylon, which had been similarly invaded in 1804 and probably often previously, suffered very severely. The spread of cholera in the island was naturally enough attributed to the commercial intercourse between Trincomalee and the infected ports on the coast of Coromandel. Whatever may be said for or against this belief as regards Ceylon, it is difficult to offer any other explanation of the outbreak which occurred the same year in Mauritius than the obvious one that it was carried over the sea by trading ships; for even though the evidence which exists that the Mauritius epidemic took its start from the arrival, with cholera on board, of the ship *Topaze*, were proved to be defective, it could scarcely be accounted for in any other way than as a result of commercial intercourse. From Mauritius cholera spread to Madagascar and the Portuguese settlements on the east coast of Africa.

In the course of 1820 cholera seems to have spread over Asia. In that year it was at Canton and Nankin, and travelled up the Yang-tse-kiang into the interior of China, and finally reached the capital. In the same year it is said that 150,000 persons died of it in the island of Java. Celebes, the Moluccas, and the Philippines were invaded at the same time. Burmah, Siam, and Singapore had been ravaged the previous year, and it was believed that the latter place, where so many streams of commercial movement meet, was the source whence the infection was distributed over China and the Malay Archipelago. The explanation was probably correct. By the universal infection of all the ports of our Indian dependencies in

1819 the channels of European commerce in the East were more thoroughly contaminated than they had ever been before. Modern experience teaches us that though cholera is very unapt to spread in this way, it may do so; and I confess it appears to me quite impossible to doubt that in those early years of its history it did so.

From 1820 onwards we have evidence that cholera has never been absent from Bengal, and has behaved throughout in the same way that it does now. The best general idea of the extent of its influence and of the differences which subsist between years of great epidemic prevalence and others, may be gained by an examination of the series of maps which have been published by the Indian Government. The conclusions which these maps suggest, and which are confirmed by the more minute and exhaustive study of cholera statistics which has been made by Dr. Bryden,* may be summarily stated as follows.

Within certain areas, the limits of which comprise the alluvial plains adjoining great rivers, and particularly in the deltas of such rivers, cholera is always present. Outside these so-called endemic areas some places are distinguished by their liability to the epidemic prevalence of the disease, others by their special immunity, and in general no relation can be traced between liability to epidemic prevalence and personal intercourse with infected districts; so that, however clear it may be that the infection of cholera is capable, under certain conditions, of being conveyed from place to place, Indian experience affords no ground for attributing any importance to such conveyance as a means of the spread of cholera in that country.

Let me now try to give an account of the circumstances which led to the escape of cholera, if such an expression may be used, from its Indian home into Europe. As probably every reader knows, the first European country invaded by cholera was Russia, and the first European town of any importance was Orenburg, on the Ural, one of the great feeders of the Caspian. How did cholera find its way from the Indian Peninsula to the Caspian? The only answer that can be given is that the communication took place by way of Persia, and that Persia itself was invaded not, as has been sometimes said, by Afghanistan, but by the Persian Gulf. In 1821—that is, a year after the epidemic of Zanzibar—there was a destructive outbreak of cholera at Muscat in Arabia and at the Persian port of Bushire, and a little later at Bagdad. From these littoral beginnings the epidemic spread during the next year (1822) over the whole of Persia and great part of Asia Minor. In 1823 it was in Damascus and Aleppo, having at the same time or previously existed in Iskanderoon and other places on the Mediterranean. It is usually stated that in 1822 cholera crossed the Caucasus for the first time, the only ground for the statement being that in that year it prevailed at about the same time at Tiflis and at Astrachan. In reality, cholera seems to have reached Astrachan, not over the Caucasus, but by creeping along the Caspian shores from Resht, which was the first place invaded. In the Caspian, as in India, it found a suitable soil in the deltas of the Terek and the Volga, and finally ascended the Ural, as has been already noted, to Orenburg. Beyond these limits cholera failed to penetrate further into Europe either by the Mediterranean, the Black Sea, or the Caspian, its disappearance in Syria and in Astrachan being simultaneous. There seems good reason for believing that it was entirely absent for six years (1823 to 1829), but in August 1829 it reappeared in Orenburg without its being possible to ascertain with any certainty whence it came. All that can be asserted is, that it was at the same time widely scattered over Central Asia, in Afghanistan, at Teheran, at Khiva and Bokhara, as well as on the shores of the Caspian, and that in consequence it was on this occasion believed to have rather come by Central Asia than from Persia.

---

* See 'Epidemic Cholera in the Bengal Presidency.' By James L. Bryden, M.D. Calcutta. 1869.

In 1830, the year after the Orenburg epidemic, cholera made its first great advance into Europe. In August of that year there were destructive epidemics at Astrachan (where there is good reason for believing that the cholera had wintered), at Zaritzin, at Saratov, at Kasan, and finally at Penza—all, with the exception of the last, on the Volga. A few weeks later it was at Taganrog, Kertch, Sebastopol, Cherson and Odessa, and finally, in September 1830, began the epidemic of Moscow, which was rendered memorable by the self-sacrifice and devotion of the Russian Emperor. In 1831 cholera for the first time spread over Central Europe. Beyond the broad fact that Russia was first invaded, it is quite impossible to say how this momentous result was brought about, as the reader may at once satisfy himself by comparing the following dates, which are derived from Dr. Peters' 'History of the Travels of Asiatic Cholera,' published in the Reports of the United States War Department:—Moscow, September 1830 to March 1831; and in the latter year, Petersburg, June; Warsaw and Cracow, April; Dantzic, March; Berlin, August; Hamburg, October. In October 1831 cholera appeared at Sunderland and became epidemic there and in the neighbouring towns, Newcastle, Gateshead, Shields; but it was not until a large number of persons had been attacked and died that it was admitted to be Asiatic. There is evidence that during the preceding summer the disease had been introduced into the port of London, and had even spread among the maritime population; but notwithstanding that no special precautions appear to have been taken, London itself remained exempt until early in the spring of 1832.

In the summer of that year it prevailed in most of the seaport towns of England and Ireland, and was carried across the Atlantic by Irish emigrants. For when, in June 1832, the disease broke out in a lodging-house in Quebec* which had received a number of these emigrants, destroyed fifty-six lives, and in the next fortnight spread everywhere in the town, it is impossible to doubt that these persons brought with them to their new home the seeds of cholera. The history of the invasion of Montreal, which occurred about simultaneously, was but a repetition of the experience of Quebec. During the autumn of 1832 and the year following, cholera ascended the St. Lawrence to Chicago, and thence found its way to the Upper Mississippi, where it very seriously interfered with the military operations against the Indians. In 1833 it appeared in Cuba, whence it spread later in the same year to Mobile, New Orleans, Tampico, and other ports on the Gulf of Mexico, and eventually to Mexico and Vera Cruz. Epidemics continued to occur in the Spanish-speaking countries of the New World until 1834–35, in the former of which years Spain itself was for the first time invaded. The great epidemics of Madrid and Barcelona were followed by a general extension along the Mediterranean coast—Cette, Marseilles, Toulon, Nice, Genoa, and Naples being attacked in the order in which they have been mentioned. As there was an interval between the Mediterranean spread and the great wave which had affected England in 1832, it seemed as if the disease, which was communicated to the New World from the Old, had been returned back to it from the West Indies. Whether this was so or not is scarcely worth inquiry. It would be much more interesting if we could explain how it was that the Mediterranean, which was in 1832 exposed to every conceivable chance of infection, was not invaded until 1834; and why, having seized upon such ports as Marseilles and Genoa, it showed no tendency to travel northwards to the country it had previously invaded. Let me add that cholera did not leave Europe until 1837, after which the Western World was free from it for a decade.

Cholera reached the Caspian for the third time in April 1847, its arrival being the outcome of a general spread of the disease in Persia and Central Asia. It soon found its way into the interior of Russia and broke out for the second time in Moscow, two months after it had appeared, almost simultaneously, at Astrachan and Constantinople. By the winter of 1847–8 it was at Riga, and spread, during the following summer, just as it had done before, along the Baltic coast, reaching Hamburg in September.

The conveyance of cholera into England, and from England to America, was but a repetition of what had happened in 1832; and the same sort of evidence existed at New Orleans and at New York, in which places the epidemic began simultaneously (December 1848) of importation by emigrants. From 1847 Western Europe was again free from cholera for six years, notwithstanding that it was always present somewhere in the East. 1853 was a cholera year: it was marked by a fearful epidemic in St. Petersburg, which again spread along the Baltic coast, reaching London and Liverpool in July, but not becoming epidemic until the following year.

---

* Dr. Peters, *loc. cit.* p. 564.

After a dozen years of immunity, cholera again appeared in Europe in 1865. On this occasion it was generally believed that the pestilence reached Europe, not as before by the Caspian and Black Sea, but by the Mediterranean. There is no doubt that cholera was rife at Jedda and Mecca in the spring of 1865, also that it prevailed from the beginning of June in Alexandria, and appeared in Malta on the 20th of that month, and about the same time at Marseilles, and subsequently on the Coast of Spain (Valencia). As was the case last summer, the seed was conveyed to Paris, and on that occasion bore fruit in the deaths of about 7000 persons in five months. There was also, as many readers will remember, a small epidemic at Southampton, the origin of which was traced by Dr. Parkes to the arrival of ships with cholera on board from Alexandria; but with this exception Western Europe remained free until the following year. Nor in all probability would England have ever suffered as it did in 1866, had the sporadic spread of cholera from the Mecca pilgrims been our only risk. At the time that all these events were going on about the Mediterranean a new storm was brewing in the old quarter —in North Germany. The appearance of cholera on August 29, 1865, at Altenburg, a place situated in the very middle of Germany, was one of the strangest events which is on record in relation to cholera in Europe. The epidemic in that district, which is exclusively watered by tributaries of the Elbe, lasted for four months (i.e. until the very middle of winter), culminating in October, and destroying 500 people. All of these deaths occurred in some half-dozen towns lying to the southward of Leipsic. This was followed by a general dissemination of cholera in Germany. By July 1866 it was already at London and Liverpool. The Prussians in their march into Bohemia passed through the country that had been the seat of the epidemic in the previous year, and on their return from their short but victorious campaign encountered it at Halle and Leipsic, in which places by that time it had gained headway, and suffered so severely that more soldiers' lives were lost by cholera than by the weapons of the Austrians. Since 1866 we in England have again had a long period of immunity, notwithstanding that we have been repeatedly threatened. In Germany a succession of epidemics occurred between 1873 and 1875, none of which reached England. Although these, from the completeness with which they were investigated, afford materials for a very instructive study of the subject, I must for the present content myself with the sketch already given of the epidemics which have affected this country.* It may, perhaps, suffice to enable the reader to see that in these successive spreads of cholera over the civilised world it follows certain general laws—as, for example, that it loves great rivers, and particularly their deltas and estuaries, and that it is capable of being conveyed over sea and land, following for the most part the lines of commercial intercourse. On either side of this general view which the unbiassed intelligent reader of cholera history finds himself compelled to take, range the opposite opinions of contagionists on the one hand, who believe that cholera came to Europe in 1830, because the *materies morbi* accidentally escaped from India; and on the other, the believers in the spontaneous origin of cholera, who think that they mean something when they say that the cause of cholera is "atmospheric" or "telluric."

---

* See Gunther, 'Die indische Cholera in Sachsen im Jahre 1865,' Leipzig, 1866; and Pettenkofer, 'Die Sächsischen Cholera-Epidemien des Jahres 1865.' Ztsch. f. Biol., 1866.

Let us now see what can be learned by looking at the subject from the consideration of its pathological nature. With this view we will take as our starting-point the assumption that cholera is a "specific" disease, which means simply that it has a particular or proper cause—a cause which is peculiar to it, and without which it cannot come into existence. In each of the diseases known as smallpox, glanders, diphtheria, cattle plague, the cause presents itself as a tangible material which can be obtained from the body of any human being or animal affected with it, and may thus be subjected to experimental investigation. In the case of the affection called woolsorters' disease, or splenic fever, to which persons engaged in manipulating particular kinds of wool imported from the East are liable, we know that the material cause not only exists in the body of the sufferer, but also in the wool by which he is infected. Cholera we believe to have a similar material and tangible cause, but no one as yet has been able to seize upon it. It has been sought for both diligently and skilfully, but it has hitherto eluded investigation. It will therefore be convenient to speak of it as the unknown entity *x*.

In the search after the *x* of cholera which now occupies so many minds, the method which the pathologist ought to follow—the only one he can follow with reasonable prospect of success—is that of proceeding step by step from the known to the unknown. Conjecture must lead the way to discovery, but those conjectures only are likely to be productive which are founded on the comparison of unknown with known relations.

The fact which we have to explain is that cholera has spread from India all over the world, and is always spreading somewhere. The knowledge we have to guide us in seeking for an explanation is that in other spreading diseases the spread consists in the conveyance of a *something* tangible from the infected person or thing to a healthy person at a greater or less distance; and the legitimate guiding conjecture is, that whatever may be known as to the nature of the conveyable something in the cases in which it can be investigated, is likely also to be true in those cases in which, as in cholera, it is for the present beyond our reach.

In the current language of pathology, the conveyable something by which infectious diseases are propagated is called *contagium*, a word which may be conveniently used, provided that it is not allowed to carry any suggestion that the disease to which it is applied spreads by personal contact or intercourse. Like other scientific terms, its use is to serve as a label for certain knowledge. Under the heading contagium, the pathologist says (1) that all contagia consist of organised (not merely organic) matter; (2) that this matter must, in order to be disseminated, be in a state of fine division (particulate); (3) that the particles of which it consists are living; (4) that they derive their life (not as having been themselves bits of the living substance of the diseased man or animal, but) from parents like themselves. With reference to all of these propositions, excepting the last, there is agreement of opinion. It is now eighteen years since it was proved by the investigations of Chauveau that all the best known contagia (which are liquids of the character of vaccine lymph) owe their activity to the minute, almost ultra-microscopical, particles which float in them; and no one doubts that these particles are organised, and that their power of producing disease depends on their organisation. Further, we know, with reference to one or two diseases—namely, woolsorters' disease, or splenic fever, tuberculosis, leprosy, and one form of septicæmia, that the particles in question are not only organised, but themselves organisms—i.e. living individuals deriving their life from parents like themselves. But from the moment that the pathologist begins to infer that because in these particular instances, which can be experimentally investigated, infection occurs by organisms, it must be so in the case, for example, of cholera, of which the behaviour is very different indeed from that of any of the infectious diseases above enumerated, he leaves certainty behind him and passes into the region of more or less probable conjecture. With reference to the special question which now interests us, he has to compare the mode of operation by which cholera spreads with the modes of operation of those diseases which are propagated by self-multiplying contagia—first, with a view to the estimation of the antecedent probability that they are essentially identical; and secondly, to the testing of the estimate arrived at by such experimental investigations as circumstances place within his reach.

The antecedent probabilities may be stated as follows:—If the reader will approach the subject with a mind freed for the moment from metaphysical considerations, he will see that the spread of cholera over the world must be due either to the dispersion of infected persons, or of things with which such persons have been in contact, or to the dissemination through the air of what may be called "cholera-dust." The question whether there is such a thing as cholera-dust rests on the teaching of experience as to whether cholera can or cannot jump from one place to another at a distance without the aid of personal intercourse. If this does occur it can only be by dust.—i.e. minute particles of infective material suspended in the air. If it is not so, it remains to be determined whether such events as the conveyance of cholera from Ceylon to Mauritius in 1819, from Astrachan up the Volga in 1830, from Hamburg to Sunderland in 1831, from Dublin to Montreal in 1832, and from Havre to Halifax in 1849, in all of which immigration from infected places of men with their belongings led to the appearance of cholera where it was before unknown, should be attributed exclusively to the introduction into these places of persons actually suffering from cholera, or to the circumstance that these persons, whether themselves infected or not, brought with them an infected environment. Experience all over the world is in favour of the latter alternative, for on the one hand it teaches that cholera is not "catching," so that attending on the sick is in itself unattended with any risk; and, on the other hand, that cholera has such a power of *haunting* localities, that a house, street, town, or district where cholera prevails to-day becomes thereby more liable to a second visitation next year than it would otherwise be. Now the only way in which such a fact as this can be explained is by supposing that the material cause of cholera is capable of existing in human belongings for a length of time independently of the human body from which it sprang. But in addition it suggests something as to the nature of that cause. That the contagium of cholera is capable, after many months of quiescence, of recovering its activity whenever the conditions of that activity come into existence, is a fact which, while it is otherwise unintelligible, is very easy explained on the supposition that the contagium itself is endowed with life; for it is characteristic of living things that they have the power of sleeping and waking—of hibernating, and reviving under the influence of summer warmth. In addition to this, we are led in the same direction by the consideration, which applies to cholera in common with all other spreading diseases, that whatever the x may be, it certainly possesses another essential property of organisms—namely, that it is capable of self-multiplication; for however inconsiderable may be the weight of material which is wanted for the infection of a single individual, it is clear that when cholera invades a country for the first time, the increase of that material, in the body of the first case, then in the bodies of the thousands subsequently affected, must be enormous.

The conjecture therefore that cholera, like other epidemic diseases, owes its power of spreading to a living and self-multiplying organism is so well founded that we are justified in taking it as a starting-point from which we may at once proceed to inquire—first, where this self-multiplication takes place; and secondly, how it is brought about. The first question, I think, I can best answer by stating to you the view on the subject which has received the most general acceptance.

In splenic fever, as we have seen, there is no doubt whatever that the disease of which the human being or the animal affected with it dies, proceeds *pari passu* with the development of the disease-producing organism x; for in the hours, be they few or many, which intervene between the sowing of the seed in the body of a living animal and the maturation of the harvest—that is, between inoculation and death—the whole of the living body of the affected animal becomes so thoroughly infested that in many instances no fragment of tissue, no single drop of circulating blood, can be found which does not contain thousands and tens of thousands of the characteristic rods (or bacilli), each of which individually is capable of communicating the disease if sown into the body of a healthy animal. So also in another well-investigated instance, that of relapsing fever, we have evidence that the multiplication of x takes place in the circulation, and that the presence there of the characteristic spirilla is so associated with the appearance of the fever itself, that the one never manifests itself without the other having preceded it.

But as regards cholera, nothing of the kind can be observed. As yet no one has been able to find the organism, either in the blood or in any living tissue, notwithstanding that the research has been conducted with every possible care. Nor has it been found either that the bodies of persons affected with cholera, or that any part of them, possessed the power of infecting other healthy persons. Consequently the opinion first arrived at and formulated by Professor Pettenkofer has come to be very generally adopted—that in cholera the multiplication of x takes place, not in the tissues of the sick person, but in his environment. Let us examine a little more closely what this means.

Under the term environment is included everything which is in relation with the external surface of the body, including the air we breathe and the water and other material which we use as food. And inasmuch as no multiplication can take place otherwise than in a suitable soil consisting of organic matter, and no such soil exists in the air, we may limit the possible seats of multiplication to the moist organic substances of various kinds which exist at or near the surface of the earth. Putting this into plainer language, it means that when the cholera x invades a previously uninfected locality in which it is about to become epidemic, the first thing it does is not to find a home for itself (as the x of smallpox, of cattle-plague, or of splenic fever would do) in the body of some healthy person, but to sow itself in *whatever material at or near the surface is fit for its reception and vegetation.*

Now, in our study of the laws of diffusion of cholera we have seen that, although cholera may be repeatedly introduced by personal intercourse into an uninfected locality without result, it finally, after a shorter or longer latency, bears fruit; and this we explain on the hypothesis that, of the two conditions which are essential to the fructification of the germ—namely, the presence of the organism itself, and the presence of a soil suitable for its growth, the latter is of more importance than the former; that, in short, the reason why a given town or country remains exempt from cholera—is not that the seed of infection fails to reach it, but that those local conditions which are necessary for its vegetation are wanting. If we call the environment y, then the cause of cholera is not $x+y$, but xy, so that whatever value we assign to x, the product disappears as y vanishes.*

If the cholera organism multiplies in the soil, not in the individual, it must, in order to exercise its disease-producing function, attack the human body by one of two channels, either by air or food; it must be taken in either by breathing or swallowing, for the skin has so little power of absorption that it need not be considered. It seems to be extremely probable that in either case x enters the organism by the same portal—namely, by the process of intestinal absorption; that is, by the same channel by which the nutritious part of our food is assimilated—i. e. that even if it were introduced by the breath, it would still act by localising itself in the alimentary canal. Consequently, if we want to engage in the search for it, there are two places where we should expect and seek to find it—namely, first, in the soil; and secondly, in the intestine of infected persons. Hitherto attention has been exclusively given to the investigation of the absorbing apparatus of the alimentary canal as the spot in which x would be likely to be caught as it were *flagrante delicto.*

In illustration of this, let me now refer to the efforts which have been made at various periods to carry out this inquiry. Without going back to the attempts made by Dr. Snow in the epidemic of 1854, I will content myself with a rapid survey of what has been done in more recent times, premising that there is no necessary connection between the notion which I am now advocating—namely, that the cholera x resides in the soil, and produces cholera by finding its way into the intestine, and the belief that the intestinal contents of persons suffering from cholera are directly pernicious and infecting.

In 1870 a morphologist of great distinction (Professor Hallier) published a remarkable series of observations, in which he endeavoured to show, on purely morphological grounds, that the birthplace (or rather the nursery) of cholera is the rice-plant—that a parasite which grows on this plant, so essential to the populations of the endemic area of Bengal, becomes in the course of successive transformations the cholera fungus; that this fungus throws off spores which are the immediate producers of cholera; and that by means of the endurance and extreme levity of these spores, they serve as agents by which cholera is spread all over by the wind; and so on. Of Hallier it is sufficient to say that, however distinguished he might be as a botanist, he was a bad pathologist, and that his method was fundamentally wrong, inasmuch as he proceeded throughout on the assumption that the morphological characters of an organism supposed to be infective may be taken as evidence of its infective nature; whereas pathology admits nothing to be a contagium unless it can be observed in action as such. For one thing, at all events, we may be grateful to the Jena botanist. It was for the purpose of investigating his theory that those indefatigable cholera workers, Drs. Lewis and Cunningham, were sent to India, where, although they spent more time and labour in correcting Hallier's mistakes than it took Hallier himself to fall into them, they were thereby afforded opportunity of acquiring information of the highest practical and scientific value. It would take too long to refer to other efforts in the same direction, but it may be readily understood that the question of the material cause of cholera was too important to be neglected, and that as soon as cholera seemed once more to threaten Europe it again urgently claimed the attention of scientific pathologists. Accordingly, in 1883, Dr. Koch, who is the author of two of the greatest discoveries of modern times in relation to spreading diseases, was deputed by the German Imperial Government to proceed to Egypt, and then to India, to investigate cholera.

---

* In designating the seed, germ, contagium, or *materies morbi* of cholera x, and the soil or environment y, I follow Professor v. Pettenkofer.

Stated in few words, the result of Dr. Koch's inquiries were—(1) That the x in cholera has the form of a curved rod, which Dr. Koch likens to a comma (as written, not as printed); and (2) That the disease (cholera) is caused by the presence, growth, and multiplication of this organism in the apparatus for absorption contained in the lower part of the small intestine, and by the consequent formation there of an animal poison which produces the collapse and the other fatal effects of cholera.

These statements, as soon as they became publicly known, assumed a very great importance, because they appeared to afford support to a doctrine with which they have no necessary connection—namely, that of the communicability of cholera by direct personal intercourse with the sick. The mere fact of the existence of countless myriads of organisms of a particular form in the intestinal liquid, although very interesting in itself, affords no evidence that they are the culprits, unless two other things can be proved respecting them—namely, that they possess the power of producing cholera wherever they exist, and that they are capable of maintaining their life, not merely within the intestine, but also in the soil; for, as we have seen, the evidence that the material cause of cholera is capable of existing outside of the body and of spreading over the world independently of the presence of persons affected with the disease, is so conclusive, that no explanation of cholera can be accepted which does not take this into account.

Now in India the question of the prevention of cholera is a very practical one. Here, cholera is chiefly a question of preserving life; in India it is one of commerce, and consequently of national prosperity. If it were believed in India that the cholera patient is himself a source of infection, that each individual comma is a source of danger, India would be compelled to adopt prophylactics of the same kind as those which were adopted last year by the ignorant and short-sighted administrators of Italy and France. And it was, I believe, on this ground judged necessary by her Majesty's Indian Government to send out a special Commission for the purpose of reporting generally on the practical bearing of the German investigations. The Commission was under the general guidance of Dr. Klein,* who was selected on the recommendation of the highest scientific authority in this country, as being the person who in England, by his previous researches, had shown himself *facile princeps* in inquiries of this nature. The finding of the Commission was, that although Dr. Koch was perfectly accurate in his statement of fact, he had gone too far in inference. In other words, that although the so-called cholera bacillus swarms in the intestine of every person affected with cholera, it does not there play the part which is attributed to it.

I shall, I think, most usefully conclude this paper by stating as clearly as I can in what way the knowledge and experience already obtained as regards the cause of the spread of cholera by the two methods of inquiry which are available for the purpose (and which for the moment I will call the epidemiological and the bacteriological) may be brought to bear on practical questions. And here I will ask the reader to note once more amid the apparent differences of opinion which exist at the present moment, as regards some questions which have lately come prominently to the front, between persons whose competency cannot be denied, that such persons are nevertheless in agreement, not only with respect to the sources of danger and the means of guarding against them, but also as to the most fundamental theoretical questions. Thus, for example, while we hesitate to admit that the particular organisms which Dr. Koch has so carefully investigated have anything to do with the causation of cholera, the conclusions arrived at nearly twenty years ago by the two leading authorities of that time—Simon in England and Pettenkofer in Germany—that cholera depends on an organism, and that its spread cannot be accounted for in any other way, are as certainly true now as they were then. But this certainty arises not from any direct evidence which has up to this time been offered with reference to a particular bacillus, but from the various facts which go to show that in places infected or haunted by cholera something else exists besides the infected persons. So that if we could imagine all the infected persons in such a locality to be removed by some act of absolute power, such an act would not stop the progress of the epidemic, for cholera would still be there.

Of the two methods of inquiry above referred to, the bacteriological applies to the nature of the contagium itself, and the epidemiological to the nature of the environing conditions which favour its development. Hitherto the investigation of the latter has been by far the most successful. But it would be a great mistake to allow the apparent failure of such researches as those of Dr. Koch in Egypt and in India to discourage the efforts which are now being made everywhere by earnest and devoted workers to accomplish what has baffled so able an investigator. Whenever the discovery is made, it will not only serve as a key to the understanding of cholera as a disease, and thereby tend to render its treatment a little less hopeless than it is at present, but it will serve as the necessary completion of the knowledge we have gained from the combined experience of the medical profession in India, in Europe, and in America, with reference to the behaviour of cholera as an epidemic disease. To make this clear, all that is necessary is to summarise statements which have been already placed before the reader in the course of this article. What we have learned is that the liability of a locality to cholera depends, first, on the physical characters of the soil; and secondly, on certain changes which it undergoes in the course of the seasons. The peculiarity of the soil which favours cholera is unquestionably want of natural or artificial drainage, combined with the presence in the liquid with which it is soaked of such organised material, derived from the tissues of plants or animals, as render it a fit soil for the development and vegetation of microphytes. The seasonal change which favours cholera is that which expresses itself in the drying of such a soil under the influence of summer temperature. In Europe this takes place in July, August, and September, in which last month, as the following table * strikingly shows, cholera attains its maximum of destructiveness:—

---

* The Commission consisted of Dr. Klein, F.R.S., and Dr. Heneage Gibbes. The Report has only just been published, but the scientific results of the inquiry were communicated by Dr. Klein to the Royal Society in February last.

Month . . .	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	March.
Mortality . .	112	416	4,392	8,480	33,640	56,561	35,271	17,630	7,254	2,317	842	214

But be it ever remembered that these two liabilities of time and place do not explain everything. No combination of soil and season, however favourable, will produce a harvest unless the seed has been sown. It holds as true now as it ever did, that "if we possessed the requisite knowledge, the disease could always be traced back in lineal descent to its origin in some poor Hindoo on the banks of the Ganges, as certainly as the pedigree of a horse or dog can be followed to his remote ancestors."

---

* The numbers express the mortality from cholera in Prussia during the thirteen years, 1848-1860.

Notwithstanding the overwhelming evidence which now exists in proof of the harmlessness of the so-called "rice-water evacuations," it is not the less certain that the mechanism by which the infection of the soil takes place (i. e. by which the disease from being epidemic becomes epichthonic) is its contamination by the discharges of sick persons. For there is no other possible way by which the soil can acquire the morbific property which facts compel us to attribute to it. Similarly, it may be regarded as absolutely certain that the influence of the soil on those who are infected by it is due to the penetration into their bodies of infective material, either by respiration or swallowing; that, in the absence of proof of "cholera-dust," it is a matter of urgent necessity to avoid the use of water which contains such material as from its chemical nature may be reasonably considered capable of harbouring infective microphytes.

In this country and in our Indian possessions experience has led us to do the very things which science, were her opinion asked, would approve as of primary importance. In Calcutta, the measures of sanitary improvement, particularly drainage works, which have been carried out under the highly efficient sanitary administration there, have during the last dozen years led to a diminution of the cholera mortality to something like a third of its previous average, and similar good results have been obtained elsewhere in India, in so far as it has yet been possible to bring about the necessary reforms. In London we have been lavish in our underground expenditure. Our water supply is good and abundant, and our subsoil is dry, so that dwellers in the west and north need not feel much apprehension even though cholera were again to fix itself in the east. But we may, I think, venture to anticipate that this year, at least, we shall not be tried. Cholera, had it intended to attack us this season, would already have been on the march. The eastern provinces of Spain are suffering severely, and it can scarcely be hoped that other parts of the Mediterranean will remain exempt; but Central Europe is free. Hitherto cholera has come to us from Holland or Germany, not from southern Europe, so that until the Rhine, the Elbe, the Oder, or the Vistula are threatened we need be in no immediate apprehension as to the Thames or the Mersey. But in venturing on this favourable forecast, I would beg the reader to understand that I speak with no authority, and recognise his competence to judge as well as I can of its value. Neither science nor experience affords a key to the reasons why cholera now follows one course, now another, in its wanderings over the world.

[J. B. S.]

WEEKLY EVENING MEETING,

Friday, May 22, 1885.

Sir William Bowman, Bart. LL.D. F.R.S. Manager and Vice-President, in the Chair.

Walter H. Pollock, Esq. M.A.

Garrick as an Actor.

The lecturer began by a brief reference to Garrick's brilliant place amid a crowd of persons distinguished in all kinds of ways who flourished at the same time with him, and to the brilliant career with the main features of which his audience was no doubt acquainted. He proposed to dwell not on them, but on the curious and minute accounts of Garrick's acting in certain parts preserved by eye-witnesses of skill and experience. Lichtenberg on Abel Drugger, on Sir John Brute, and on Hamlet, was quoted in a translation, and his account was compared with that given by Davies, the friend of Johnson, who was an actor and a bookseller, a man of singular talent, reading, and judgment, and the biographer of Garrick as well as the author of a book full of interest called 'Dramatic Miscellanies,' from which many passages were quoted. Other authorities were referred to in the same way and with the same kind of illustration that was given to these two. Garrick's dramatic alterations of Shakspeare were referred to and passages given from one of the worst of them, the Romeo and Juliet. It was at the same time pointed out that Garrick had done much for other plays of Shakspeare in the way of restoring the original text to the stage. The lecturer concluded by citing passages from the Garrick Correspondence, which seemed to prove conclusively that Garrick was what we now call a "mannered" actor—and he devoted the last part of his lecture to arguing that so-called "mannerism," in other words individuality, is not necessarily a bar to the faculty of impersonation of a very various kind; that it is, on the contrary, when allied with genius, in other arts as in acting, the very quality which engages and keeps attention. "Out of being nothing but yourself" the lecturer said, "nothing can come. Out of being everything but yourself nothing can come. Genius hits the mark between the two and commands success and admiration."

[W. H. P.]

WEEKLY EVENING MEETING,

Friday, May 29, 1885.

William Huggins, Esq. D.C.L. LL.D. F.R.S. Manager and Vice-President, in the Chair.

J. J. Coleman, Esq. F.C.S. F.I.C. and Professor J. G. McKendrick, M.D. LL.D. F.R.S.

The Mechanical Production of Cold and the Effects of Cold upon Microphytes.

This discourse was delivered by Mr. J. J. Coleman, and embraced two parts, viz. 1st, a description of his own researches and their application in the construction of cold-producing machines; and 2nd, a description of a joint research undertaken by Professor McKendrick and himself upon the effects of very low temperatures upon microphytes.

Mr. Coleman pointed out that in the first place the title of the discourse might be objected to, inasmuch as it was scarcely correct to speak of the "production of cold." As, however, long usage had made the phrase conventional it would be used in the sense of meaning production of a state of coldness.

A close examination of all known methods of producing cold by artificial means involved the employment of some volatile liquid or compressed vapour capable of spontaneously expanding. Thus, commencing with water, its spontaneous evaporation produced the state of coldness of the domestic water-cooler.

The evaporation of ether, as in the spray used by surgeons; also the evaporation of liquid sulphurous acid, which boiled at − 10° C.; of liquid carbonic acid, which boiled at − 78° C.; of liquid nitrous oxide, which boiled at − 86° C.; of liquid ethylene, which boiled at − 102° C.; or of liquid air, which boiled at − 191° C., were all perfectly analogous to the spontaneous expansion of compressed air, when released from pressure by opening the cock of a vessel containing it.

The nearer the compressed vapour is to the state of a perfect gas, or of a hypothetical perfect gas, the more exactly is the heat absorbed by expansion balanced by the heat generated by the friction of the molecules before coming to rest. This followed from the joint researches of Sir William Thomson and Dr. Joule conducted thirty years ago.* These eminent men, in a classical series of experiments, the description of which occupies 122 pages of Sir

---

* "On the Thermal Effects of Fluids in Motion." See collected papers of Sir W. Thomson, vol. i. Cambridge University Press, 1882.

William Thomson's researches, and involved upwards of 1000 observations collected in 50 tables, demonstrated among other things that air having a pressure of 100 lb. per square inch, when passed through a porous plug so as to reduce its pressure to that of the atmosphere, only becomes cooled to the extent of 1·6° C., hydrogen gas similarly expanded being heated 0·116° C.

Free expansion of atmospheric air under pressure through a small orifice of any kind was similar in its results, which Mr. Coleman demonstrated by expanding a cubic foot of air of several atmospheres pressure into a glass reservoir or chamber containing a delicate air thermometer, which was not in the least affected, although the result was thrown upon the screen magnified and illumined by a beam of electric light.

It was otherwise, however, when the experiment was made as originally conducted by Gay Lussac and Dr. Joule, in which case the vessel containing the air being expanded became cold, and the vessel into which the air flowed became hot, the one phenomenon neutralising the other.

Mr. Coleman showed by an arrangement of his air thermometer, the index of which was projected upon the screen, that the vapour which exists in an ordinary bottle containing a little liquid ether has sufficient tension to cause friction and production of heat in escaping through a narrow orifice.

In this case the heat abstracted from the evaporating liquid was compensated by the latent heat absorbed in conversion of the ether liquid into ether gas + the heat caused by the *vis-viva* of the escaping molecules coming into contact with the walls of the orifice and afterwards with the external atmosphere.

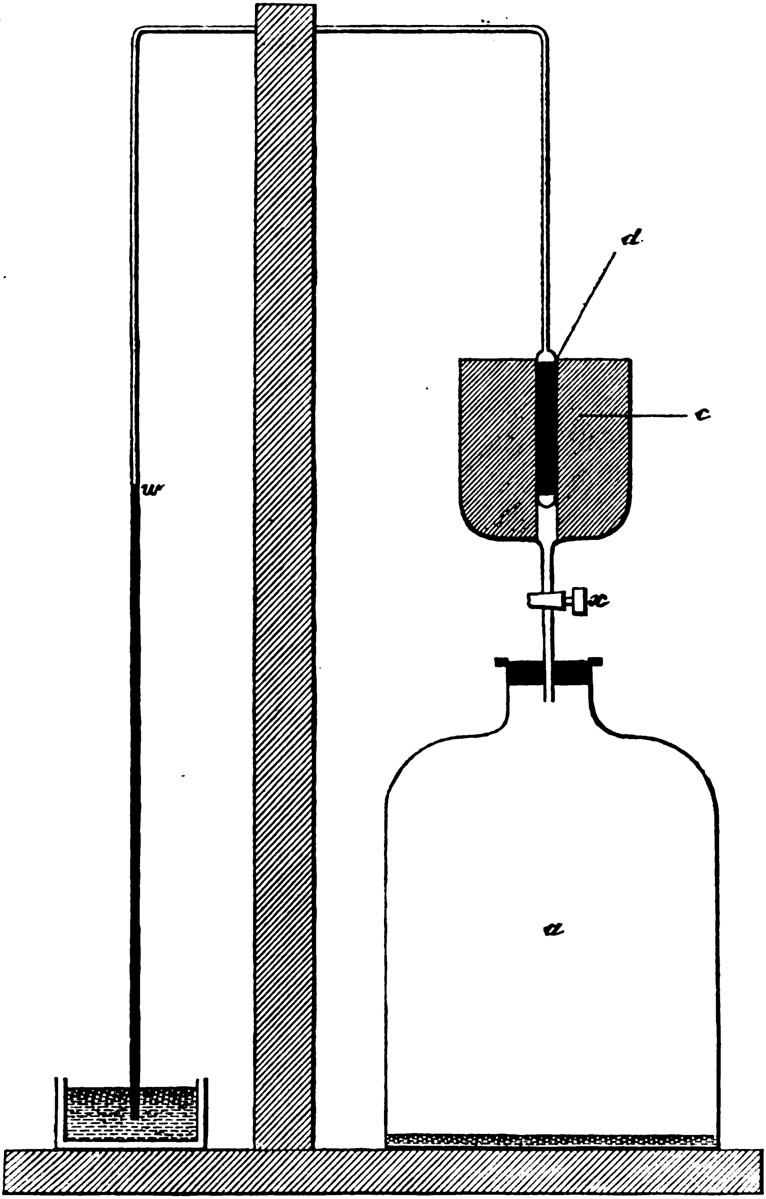
The apparatus used was called a "Tripatmoscope" and was constructed as follows:—

a is a bottle of say ⅛ of a cubic foot capacity containing a small quantity of liquid ether, and into the neck of which was fitted by an indiarubber cork a glass funnel. *c* is a plug of boxwood cemented into the funnel bowl, and having an annular aperture barely sufficient to admit the thin copper cylindrical bulb *d* of an air thermometer say ½-inch diameter and 6 inches long and packed tight into the boxwood by making the bulb a little sticky with mastic varnish and winding round it fine cotton thread. On opening the cock *x* at intervals of a few minutes, a marked fall of the water index *w* occurred which was magnified and illumined by a beam of electric light—indicating nearly 1° F. produced by friction of escaping vapour.*

The statement that is met with in some text-books, that no cold can be produced by expanding air without *mechanical work* being performed, is not strictly correct, as one fraction of a given volume of air can be cooled by blowing the other fraction into the atmosphere; but all such methods of producing cold are, however, from an engineering point of view, wasteful, for to make an economical machine energy must be abstracted from the expanding medium (generally

---

* This instrument works best in an atmosphere of about 70° F., but with a slight occasional agitation of the liquid ether gives good results at 60°.



in the form of mechanical power, but not necessarily, as for instance in the ammonia absorption machine) before the *whole* volume of the gaseous medium can be reduced in temperature, and not only is this necessary, but in order to enable the medium, when it has arrived at atmospheric temperature, to be employed for a fresh cycle of operations, energy must be introduced and then rejected.

In the case of atmospheric air the energy used in compression appears as heat every cycle which has to be rejected, and this is accomplished either by washing the air with injected water or by passing it through tubes surrounded by water.

Some idea of the heat required to be removed each cycle is afforded by the fact that compression of air to the extent of 65·86 lb. per square inch above the atmosphere increases its temperature by 332° F.

It is now about five years since machinery of this kind came into extensive use, and for a long time there was a curious controversy amongst engineers as to the two methods of removing the heat produced by compression. It was maintained that the water injection would wet the air, but the lecturer showed by expanding a cubic foot of air at 100 lb. pressure per square inch standing over water in the theatre of the Institution, that on emerging from the iron bottle containing it, it was exceedingly dry, which indeed followed from the fact discovered by Dalton that saturated vapour is liquefied by compression at a constant temperature, in direct proportion to the degree of compression. In practice, however, especially at sea, the air pressure is kept about 50 lb., and the dryness of the air is further ensured by passing it, on its way to the expansion cylinder, through an interchanger, around the outside of which the partially cold air flowing to the compressor is made to circulate.

A number of large diagrams and a complete model were shown, showing the general construction of the cold-air machines used for importing meat into Great Britain, all of which were now manufactured after the general designs introduced by the lecturer a few years ago and described to the Institution of Civil Engineers in his paper read there in February 1882.

By means of these machines Australian, River Plate and New Zealand mutton comes into Great Britain at the rate of 18,000 sheep or 400 tons per week, which represents a weekly procession a mile long and ten abreast. The quantity of American meat carried by the machines is still larger, so that the British householder is now supplied by them with meat to the value of between 4,000,000*l.* and 5,000,000*l.* *per annum*, calculated at the price of meat in retail shops.*

These machines, in an ordinary way, supply streams of atmospheric air cooled to about 80° below zero F. (– 63° C.); but, by certain modifications, they can be adjusted to deliver the air cooled much lower, and, in point of fact, to as low temperatures as have yet been produced in physical researches.

---

* According to the Smithfield Market reports, 27,007 tons of mechanically cooled meat arrived from U. S. America in 1884, and 5500 tons Australian, New Zealand, and River Plate frozen mutton in first three months of 1885.

For the purposes of the conjoint experiments with Professor McKendrick, a machine, worked by a gas motor engine, capable of delivering 30 cubic feet of air (·84 cub. metre) per minute, was employed, the cold air being made to pass upward in a square vertical shaft of wood, in the sides of which were apertures regulated by valves, and by means of which about a dozen chambers, each of 3 cubic feet capacity (·084 cub. metre), could be maintained at any particular temperature desired. These temperatures were carefully taken by an absolute alcohol thermometer, made by Negretti and Zambra, and checked by a special air thermometer devised by Mr. Coleman.

The experiments consisted in exposing for hours to low temperatures putrescible substances in hermetically sealed tins or bottles, or in flasks plugged with cotton wool; the tins or flasks were then allowed to thaw, and were kept in a warm room, the mean temperature of which was about 80° F. (27° C.); they were then opened and the contents submitted to microscopical examination with magnifying powers of from 250 to 1000 diameters. The general results were as follows:—

1. Meat in tins (4½ inches in diameter, and 1 inch in depth), exposed to 80° below zero F. (– 63° C.) for six hours, underwent putrefaction with generation of gases.*

2. Whilst these experiments were going on, comparative experiments were made with tins and bottles containing meat, but not exposed to cold. Under such circumstances, the effects of storing them in the warm room for even the short period of a fortnight, was to develop such large quantities of extremely fœtid gas, and very active bacteria and vibrios, that there was no doubt whatever but that exposure to a cold of 80° below zero (63° C.) had checked their development to some extent in the subsequent exposure to a warm temperature.

3. On the 24th of December, 1884, thirty samples of fresh meat were placed in 2-oz. white glass phials. These were carefully corked with corks previously steeped in mastic varnish, and the necks of the corked bottles were then immersed in melted sealing-wax. These bottles were divided into five sets, and marked A, B, C, D, and E, and they were treated as follows:—

A. 6 samples were exposed to zero F. (– 17° C.) for 65 hours.
B. 6 „ „ – 20° F. (– 29° C.) „
C. 6 „ „ – 30° F. (– 34° C.) „
D. 6 „ „ – 40° F. (– 40° C.) „
E. 6 „ „ – 80° F. (– 62° C.) „

---

* July 1885.—At this date, which is eight months from the commencement of the experiment, several of the sealed-up tins are on hand; the bulging of the tins and apparently the generation of gases ceased after the first month, the organisms being apparently rendered inactive by their own effluvia, or from want of oxygen. There is now very little smell on opening the tins, but a large number of organisms are visible with a ⅛-inch object-glass—alive and active.—[J. J. C.]

These experiments ended on 28th December. On the 29th one bottle from each group was again exposed to − 80° F. (− 63° C.) for six hours, then again frozen for six hours at −80° F. (− 63° C.). The whole of those were removed to the room, but in the meantime it was noticed that at temperatures below zero, and particularly so low as −80° F. (− 63° C.), the meat assumed a peculiar dirty-brown appearance. In the course of a few hours, however, the whole of the samples assumed at normal temperatures the well-known reddish colour of meat. In all cases, however, in the course of ten or twelve hours after removal to the warm room, signs of putrefaction were visible, and in the course of a few days, the putrefactive process was fully established. It is important to notice that the temperature reached in these experiments, namely, from −70° to −80° F. (− 56° to − 63° C.), is about the minimum degree of cold hitherto observed in Polar expeditions.*

4. It is well known that freezing muscle taken from a newly-killed animal, prevents the coagulation of muscle-plasma, and that the plasma can, on partial thawing, be squeezed out of the muscle and allowed to coagulate. It occurred to the lecturers that if muscle were suddenly exposed to extreme cold, before cadaveric rigidity had set in, some change might be observed in the putrefactive process. Accordingly a rabbit was instantaneously killed, portions of its muscles were at once placed in stoppered bottles and transferred to the cold chamber, then having a temperature of about − 80° F. They were kept there for ten hours; then allowed to thaw partially in the cold chamber, whilst the cold-air machine was not at work; then again frozen for twelve hours; and finally transferred to the warm room. In these circumstances, they underwent rapid putrefaction. The samples seemed to be more moist than other specimens of ordinary butcher meat, and they certainly underwent more rapid putrefaction.

5. A further set of experiments with meat was carried out, in which the samples were continuously exposed to a temperature of from − 90° to − 120° F. (− 83° C.) for 100 consecutive hours; the bottles were then removed to the warm room, with the result that in ten or twelve hours the putrefactive process seemed to be fully established.

5a. It has been shown by Pasteur † that if putrescent or fermenting substances are sealed up in a comparatively small space containing air, the processes are arrested when all the oxygen has been used up, and the products of putrefaction may undergo no further alteration. In these circumstances, in such experiments as ours, the apparent arrest of putrefaction in sealed vessels might have been attributed to the action on the organisms of the low temperature to which they had been exposed, instead of to the real cause—the removal of all the oxygen from the confined air. To meet this difficulty the importance was seen of testing the effect of cold on putrescible substances placed in test tubes and flasks firmly plugged with cotton-wool, through which there might be a free play between the gases in the tube or flask and the surrounding atmosphere. Nor was it necessary in such experiments to sterilise the cotton-wool by heat, as must be done in all researches on the effects of high temperatures, because if a low temperature were fatal to micro-organisms, it would kill those in the cotton-wool as well as those in the putrescible substances. Many experiments were made with tubes and flasks stopped with cotton-wool plugs instead of being hermetically sealed, but there was no difference in the general result.

---

* The lecturers have been favoured with the following remarks by Mr. Alexander Buchan, the eminent meteorologist, to whom they applied for information. "So far as I am aware, or can discover, the temperature of − 73·7° F. registered on board the *Alert* in March 1876, is the lowest temperature yet observed anywhere in the free atmosphere. The lowest *mean monthly* temperature known is − 55·8° F. for January, at Werchojansk (lat. 67° 34′ N., and long. 133° 51′ E.), in north-eastern Siberia." It is possible that one or more of the individual observations that make up this low mean may have given a reading lower than − 73·7° F.

† Comptes Rendus, lvi., 734-1189. See also article "Fermentation," Watt's 'Dictionary of Chemistry,' First Supplement, p. 612.

6. Six flasks were filled with fresh urine, and plugged with cotton-wool, on the 10th of December. The first one plugged with wool was exposed to the temperature of the engineering shop where the experiments were carried on (about 50° F.), and on the 13th the urine was muddy; on the 18th it was found to be swarming with bacteria and vibrios. The second was exposed for eight hours to zero F.; on the 13th it showed slight muddiness, and on the 18th it was swarming with bacteria. The third was exposed to a temperature of – 10° F. for eight hours, and on the 18th it was also swarming with bacteria. The fourth was exposed to – 20° F. with the same result. The fifth was exposed to – 30° F. with a like result. The sixth was exposed to – 80° F., and it did not become muddy until the 22nd, that is, twelve days after the beginning of the experiment. These results showed that freezing at very low temperatures delayed the appearance of the alkaline fermentation due to organisms, but a temperature of – 80° for eight hours did not sterilise the urine.

7. Samples of fresh milk, exposed to temperatures of from zero to – 80° F. for eight hours, curdled, and showed the well-known *Bacterium lactis*, and, so far as could be observed, freezing did not delay the process after the flasks were kept at a temperature of about 50° F.

8. Samples of Prestonpans beer (containing about 2 per cent. of alcohol) were similarly treated. Exposed to the air of the shop, a scum of torulæ made its appearance in three days. Freezing undoubtedly delayed the appearance of these in flasks plugged with cotton-wool, and the delay corresponded to the fall of temperature, so that the sample exposed to – 80° F. did not show the scum for

twenty-two days after its removal from the cold chamber. Still it could not be said that this degree and duration of cold sterilised the fluid.

9. Samples of sweet ale behaved in a precisely similar manner.

10. Samples of meat juice, made by boiling lean meat, filtering, and carefully neutralising, were also operated on, both in flasks hermetically sealed and having the necks stuffed with cotton-wool. Exposed to temperatures of from zero to − 80° F. for eight hours, all of these in due time showed, under the microscope, numerous bacteria, but the freezing process undoubtedly delayed their appearance, and this was most marked in the samples exposed to the lowest temperatures.

11. Samples of neutralised vegetable infusion behaved in a similar way.

12. Many experiments were made with putrefying fluids, full of bacteria and other micro-organisms. The method followed was to examine the fluid with the microscope, and to note the appearances of the organisms. Then portions of the fluid were placed either in a flask plugged with cotton-wool or in a hermetically-sealed flask, and exposed to the lowest temperature attainable—namely, − 120° F. In one set of experiments such organisms were exposed to − 120° F. for 100 consecutive hours. The thawed fluid was again examined microscopically, with the result of showing that the organisms were motionless. Still it could not be asserted that they, or at all events their spores, were dead, as, after exposure to a temperature of 80° F. for a few hours, the fluid was found to be again teeming with organisms in active movement. The conclusion arrived at was that such prolonged exposure to cold did not kill them all, probably leaving spores unaffected.

13. It was also attempted, by *repeated* freezings and thawings, to kill micro-organisms, as it was conceivable that cold might kill the adults only, leaving the spores unaffected. If, then, the spores were killed as they approached maturity, and before they had produced new spores, it might be possible to sterilise the fluid. All attempts in this direction were unsuccessful.

14. Experiments were also made with gelatinous infusions of meat, to which grape sugar had been added. Exposure to low temperatures and thawing did not destroy the gelatinous character of the substance, but putrefaction was not prevented. Such gelatinous masses, after exposure for 100 consecutive hours to − 120° F., and subsequently for fifteen to twenty hours in a warm room of 80° F., became filled with bubbles of imprisoned gas, each bubble being the outcome of one or more organisms.

15. It is a striking consideration that freezing at low temperatures makes a mass of organic matter solid throughout, so that it can only be broken to pieces by violent blows of a hammer. Beef has then a fractured surface like a piece of rock. Mutton is friable.

Still, when such a mass—say a piece of muscle—is thawed, its microscopical structure seems to be unaltered. All that can be said is that it is moister than ordinary fresh muscle. It is probable, therefore, that the bodies of micro-organisms are also frozen solid, and yet they apparently may live for a long time in this condition. One cannot suppose that in these circumstances any of the phenomena of life take place; the mechanism is simply arrested, and vital changes may again occur when the conditions of a suitable temperature return. Such considerations led the lecturers to examine whether any of the vital phenomena of higher animals might be retained at such low temperatures. It was ascertained that a live frog may be frozen quite solid throughout at a temperature of from – 20° F. to – 30° F. in about half an hour. On thawing slowly, in two instances, the animal completely recovered. When kept in the cold chamber longer than half an hour, the animal did not recover, but the muscles and nerves were still irritable to electricity, responding to weak induction shocks. Reflex action, however, was abolished. In two cases, frogs were exposed for twenty minutes to a temperature of – 100° F. On thawing, they did not recover, but the muscles still feebly responded to electrical stimulation, showing that their irritability had not disappeared. The probability is that longer exposure to this temperature, or exposure for a shorter time to a lower temperature, would destroy muscular and nervous irritability, but it is a striking fact that irritability can survive to any degree a transition through a state of solidity produced by cold.*

16. One experiment was performed on a warm-blooded animal—a rabbit. Before the experiment, the temperature of the rectum was 99·2° F., pulse 160 per minute, respirations about 45 per minute. At 10·30 A.M. it was placed in the cold chamber, the thermometer of which stood at – 93° F. At 11 A.M. it was removed for a minute or two; it did not seem to be affected, but the temperature of the rectum was now 94·2°, a fall of 5° in half an hour. It was then re-introduced into the cold chamber, the temperature of which was read off at – 100° F. It was taken out at 12 noon; it seemed to be comatose; reflex action was abolished; there were jerking movements of the limbs; its rectal temperature was now 43° F., a fall of 51° during the hour; its pulse was 40 per minute, being a fall of 120; and its respirations were barely perceptible. It was placed in a warm place, and it began slowly to recover. In fifteen minutes its temperature had risen to 72° F., in ten minutes more to 89° F. Its pulse beats when removed from the chamber were 40 per minute, in fifteen minutes they had risen to 60 per minute, and in fifteen minutes more to 100 per minute. The animal completely recovered. When removed from the chamber at 12 noon, although reflex action

---

* Kühne observed that a frozen frog's muscle will contract after thawing, but the temperatures he reached were not low.

was abolished, the muscles were still irritable to electrical stimulation, and on placing the wires over the sciatic nerve without cutting the skin, strong spasms of the muscles of the leg were caused, showing that the nerve was still irritable. It follows, therefore, that some of the effects of the extreme cold were due to inactivity of the nerve centres. Consciousness and reflex action were abolished, owing to inactivity of the grey matter of the encephalon and of the spinal cord.

The effect of the extreme cold on the warm-blooded or homoiothermal animal, as contrasted with its effect on the cold-blooded or poikilothermal animal, is very striking. The cold-blooded frog became as hard as a stone in from ten to twenty minutes, and the temperature of its body was probably the mean temperature of the chamber; the warm-blooded animal produced in itself so much heat as enabled it to remain soft and comparatively warm during exposure of an hour's duration to − 100° F. Still its production of heat was unequal to make good the loss, and every instant it was losing ground, until, at the end of the hour, its bodily temperature had fallen about 56° F. below its natural temperature. Had it been left in the chamber long enough, its bodily temperature would have fallen until it reached the temperature of the cold chamber, and it would then have become as hard as the frozen frog. It is remarkable, however, that even at the end of an hour's exposure to − 100° F., its bodily temperature was 143° above − 100° F. As blood freezes and the hæmoglobin crystallizes at about 25° F., had the temperature of the body fallen below that point, the animal would not have recovered, as its blood would have been destroyed.

The lecturer observed that several researches had been made, prior to those of himself and Professor McKendrick on the influence of cold, none of them, however, very decisive as regards the microphytes concerned in the putrefactive processes. Thus, before 1872, we find Dr. Ferdinand Cohn [*] stating that he had subjected bacteria to low temperatures without destroying their activity. He gives the temperatures as follows:—Exposure for 12 hours 30 minutes to a temperature 0° C.; for 1h. 30m. to − 16° C.; for 1h. 45m. to − 17° C.; for 3h. 30m. to − 18° C.; for 4h. 30m. to − 18° C.; for 5h. to − 17·5° C.; for 6h. to − 14° C.; and for 7h. 30m. to − 9° C. He produced the cold by freezing mixtures, and the lowest temperature he obtained was − 18° C. = 0° F. In 1870–71, M. Melsens exposed yeast and vaccine lymph to very low temperatures (− 78° C.), by means of solid carbonic acid, without destroying the power of fermentation or inoculation.[†]

Klein [‡] states that "Freezing destroys likewise most bacteria, except the spores of bacilli, which survive exposure to as low a temperature as − 15° C., even when exposed for an hour or more." Again, in another place,* he says: "Exposing the spores of anthrax-bacillus to a temperature of 0° to − 15° C. for one hour did not kill them."

---

[*] Cohn's Beiträge zur Biologie der Pflanzen, 1870, Zweites Heft, p. 221.

[†] Melsens, Comptes Rendus, Tome lxx., 1870, p. 629; also Comptes Rendus, Tome lxxi., p. 325.

[‡] Klein, Micro-organisms, p. 35.

In 1884 a remarkable series of experiments were described to the French Academy, by MM. R. Pictet and E. Yung.† These observers sealed up in small glass tubes fluids containing various kinds of microphytes, and placed them in a wooden box. The box was in the first place submitted for 20 hours to a cold of − 70° C., produced by the evaporation of liquid sulphurous acid in vacuo. The box was then surrounded by solid carbonic acid for 89 hours, and a cold of from − 70° to − 76° C. was thus obtained. Finally, the box was subjected for a third period of 20 hours to a cold produced by the evaporation of solid carbonic acid in vacuo—the temperature being estimated at from − 76° to − 130° C.—that is, a minimum temperature of 202° below zero F. They sum up by stating that the organisms were acted on by a cold of − 70° C. for 109 hours, followed by a temperature of − 130° for 20 hours. The organisms tested were *Bacillus anthracis*, *Bacillus subtilis*, *Bacillus ulna*, *Micrococcus luteus*, and a micrococcus not determined. *Bacillus anthracis* retained its virulence when injected into a living animal. The vitality of the others was not affected. Experiment showed that, whilst cold seemed to kill some of the micrococci, a great number resisted it. Yeast showed no alteration under the microscope, but it had lost its powers of fermentation. Vaccine lymph exposed to the low temperatures did not produce a pustule on the left arm of an infant, whilst another sample of the same lymph introduced into the right arm of the same child produced a pustule. Pictet and Yung conclude, from their experiments, that, in the conditions of cold indicated, many of the lower organisms were not destroyed.‡

From this concensus of evidence, Mr. Coleman observed, it then appears that any hope of permanently sterilising meat by cold (the counterpart of Appert's process by heat) must be abandoned, but it is quite possible that at some point near absolute zero the vitality of all microphytes may be destroyed.

The persistency, however, of their vitality between great extremes of temperature, ranging in fact through 400° F. is very remarkable, and is in marked contrast to their susceptibility to destruction by moist ozone, peroxide of hydrogen, and by sunlight or diffused daylight.*

---

* Klein, *op. cit.*, p. 73.

† Comptes Rendus, Tome xcviii., No. 12 (24 Mars, 1884), p. 747.

‡ In a letter to Dr. McKendrick, Professor Arthur Gamgee states that some months ago he exposed putrescible fluids to moderate degrees of cold without thereby preventing putrefaction, and that he abandoned the research as unlikely to lead to any important result with the temperature he had at command. It is also stated in Landois' Physiology, translated by Stirling, vol. i., p. 456, on the authority of Frisch, that "bacteria survive a temperature of − 87° C.; yeast even − 100° C."

There can be no doubt about disease germs being pestiferous, but it is quite possible the function of many microphytes is beneficent, preventing the undue accumulation of dead organic matter, though it is not quite clear that they are absolutely essential as a preliminary to its oxidation, and on this and kindred subjects there is much work yet to be done by the united labour of the physicist, chemist, and physiologist.

[J. J. C.]

---

* 'Researches on the Effect of Light upon Bacteria and other Organisms,' by Arthur Downes, M.D., and J. P. Blunt, M.A., Proc. Roy. Soc. xxvi. 1877.

GENERAL MONTHLY MEETING

Monday, June 1, 1885.

The DUKE OF NORTHUMBERLAND, D.C.L. LL.D. President,
in the Chair.

Raphael Meldola, Esq. F.C.S. F.I.C.
Colonel George Swinton, R.E.

were elected Members of the Royal Institution.

The following Letter was read:—

MARLBOROUGH HOUSE,
PALL MALL, S.W.,
11 *May*, 1885.

DEAR SIR,

I am desired by Prince Albert Victor and Prince George of Wales to acknowledge the receipt of your letters of the 4th instant, and to request you to be good enough to convey to the Members of the Royal Institution of Great Britain their Royal Highnesses' best thanks for having elected them Honorary Members of the Institution.

I remain, dear Sir,
Yours very faithfully,
M. HOLZMANN.

Sir Frederick Bramwell, F.R.S.
Honorary Secretary, R.I.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.—

FROM

The French Government—Documents Inédits sur l'Histoire de France: Lettres de Catherine de Médicis. Par Comte H. de la Ferrière. Tome II. 1563–6. 4to. 1885.
Receuil des Chartres de l'Abbaye de Cluny. Par A. Bruel. Tome III. 987–1027. 4to. 1884.

Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quarta, Vols. I.–IV. and V. Nos. 1–4. 8vo. And Disegni. fol. 1881–5.

Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Rendiconti. Vol. I. Fasc. 9, 10, 11. 4to. 1885.

Academy of Natural Sciences, Philadelphia—Proceedings, 1885, Part 1. 8vo. 1885.

Agricultural Society of England, Royal—Journal, Second Series, Vol. XXI. Part 1. 8vo. 1885.

American Philosophical Society—Proceedings, No. 116. 8vo. 1884.
Register of Papers in Transactions and Proceedings. 8vo. 1884.

Astronomical Society, Royal—Monthly Notices, Vol. XLV. No. 6. 8vo. 1885.

Atheneo do Porto—Revista Scientifica, No. 4. 8vo. 1885.

Bankers, Institute of—Journal, Vol. VI. Part 5. 8vo. 1885.

British Architects, Royal Institute of—Proceedings, 1884–5. Nos. 13, 14. 4to.

British Association for the Advancement of Science—Report of Meeting at Montreal, 1884. 8vo. 1885.

Chemical Society—Journal for May, 1885. 8vo.

Civil Engineers' Institution—Minutes of Proceedings, Vol. LXXIX. 8vo. 1885.

Duka, Theodore, M.D. F.R.C.S. M.R.I. (the Author)—Life and Works of Alexander Csoma de Körös. 8vo. 1885.
Declaration by Soldiers of the Hungarian Army (1848–9). Translated by A. J. Patterson. 8vo. 1884.
Editors—American Journal of Science for May, 1885. 8vo.
Analyst for May, 1885. 8vo.
Athenæum for May, 1885. 4to.
Chemical News for May, 1885. 4to.
Engineer for May, 1885. fol.
Horological Journal for May, 1885. 8vo.
Iron for May, 1885. 4to.
Nature for May, 1885. 4to.
Revue Scientifique for May, 1885. 4to.
Science Monthly, Illustrated, for May, 1885. 8vo.
Telegraphic Journal for May, 1885. 8vo.
Franklin Institute—Journal, No. 713. 8vo. 1885.
Geographical Society, Royal—Proceedings, New Series, Vol. VII. No. 5. 8vo. 1885.
Geological Society—Quarterly Journal, No. 162. 8vo. 1885.
Johns Hopkins University—University Circular, No. 39. 4to. 1885.
American Chemical Journal, Vol. VII. No. 1. 8vo. 1885.
Studies in Historical and Political Science, Third Series, No. 4. 8vo. 1885.
Linnean Society—Journal, No. 137. 8vo. 1885.
Mechanical Engineers' Institution—Proceedings, No. 2. 8vo. 1885.
Meteorological Office—Meteorological Observations at Stations of the Second Order for 1880. 4to. 1885.
Quarterly Weather Report, 1877, Part 2. 4to. 1885.
Hourly Readings, 1882, Part 4. 4to. 1885.
Numismatic Society—Chronicle and Journal, 1885, Part 1. 8vo.
Odontological Society of Great Britain—Transactions, Vol. XVII. Nos. 6, 7. New Series. 8vo. 1885.
Pharmaceutical Society of Great Britain—Journal, May, 1885. 8vo.
Photographic Society—Journal, New Series, Vol. IX. No. 7. 8vo. 1885.
Royal Dublin Society—Transactions, Vol. III. Nos. 4–6. 4to. 1884–5.
Proceedings, Vol. IV. Parts 5, 6. 8vo. 1884–5.
Royal Society of London—Proceedings, No. 236. 8vo. 1885.
Smithsonian Institution—Second Report of the Bureau of Ethnology, 1880–1. By J. W. Powell. 4to. 1883.
Society of Arts—Journal, May, 1885. 8vo.
Tasmania Royal Society—Report for 1884. 8vo. 1885.
Telegraph Engineers, Society of—Journal, Vol. XIV. No. 56. 8vo. 1885.
United Service Institution, Royal—Journal, No. 128. 8vo. 1885.
University of London—Calendar for 1885–6. 8vo.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1885: Heft 4. 4to.
Victoria Institute—Journal, No. 73. 8vo. 1885.

WEEKLY EVENING MEETING,

Friday, June 5, 1885.

THE DUKE OF NORTHUMBERLAND, D.C.L. LL.D. President, in the Chair.

PROFESSOR DEWAR, M.A. F.R.S. *M.R.I.*

Liquid Air and the Zero of absolute Temperature.

(Abstract deferred.)

GENERAL MONTHLY MEETING,

Monday, July 6, 1885.

WILLIAM HUGGINS, Esq. D.C.L. LL.D. F.R.S. Manager and Vice-President, in the Chair.

Sir Isaac Lowthian Bell, Bart. F.R.S. F.C.S. M.I.C.E.
Edwin Drew, M.D. B.Sc.
John Montague Spencer Stanhope, Esq. J.P.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following donation to the Fund for the Promotion of Experimental Research:—

The Hon. Sir William R. Grove, 25*l.*

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:

FROM

The Governor-General of India—Geological Survey of India: Records, Vol. XVIII. Part 2. 8vo. 1885.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. Vol. I. Fasc. 12. 8vo. 1884–5.

Memorie della Classe di Scienze Morali, Storiche e Filologiche. Serie 3a, Vols. 8, 10, 11. 4to. 1883.

Memorie della Classe di Scienze Fisiche, Mathematiche e Naturali. Vols. 14–17. 4to. 1883–4.

Antiquaries, Society of—Archæologia, Vol. XLVIII. Part 2. 4to. 1885.

Astronomical Society, Royal—Monthly Notices, Vol. XLV. No. 7. 8vo. 1885.

Bankers, Institute of—Journal, Vol. VI. Part 6. 8vo. 1885.

Berdoe, Edward, Esq. (*the Author*)—Browning as a Scientific Poet. 8vo. 1885.

British Architects, Royal Institute of—Proceedings, 1884–5, No. 15. 4to.

L'Institut Royal des Architectes Britanniques. 7e Conference, 1884. Cantorbéry-Londres. Par C. Lucas. 8vo. 1885.

British Museum (*Natural History*)—List of Cetacea. 8vo. 1885.

Canada, Geological and Natural History Survey of—Reports of Progress. With Maps, &c. 1882–4. 8vo. 1885.

Catalogue of Canadian Plants, Part II. By J. Macoun. 8vo. 1884.

Chemical Society—Journal for June, 1885. 8vo.

Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (*the Editor*)—Journal of the Royal Microscopical Society, Series II. Vol. V. Part 3. 8vo. 1885.

Dax: Société de Borda—Bulletins, 2e Serie, Dixième Année, Trimestre 2. 8vo. 1885.

East India Association—Journal, Vol. XVII. No. 4. 8vo. 1885.

Editors—American Journal of Science for June, 1885. 8vo.
Analyst for June, 1885. 8vo.
Athenæum for June, 1885. 4to.
Chemical News for June, 1885. 4to.
Engineer for June, 1885. fol.
Horological Journal for June, 1885. 8vo.
Iron for June, 1885. 4to.
Nature for June, 1885. 4to.
Revue Scientifique for June, 1885.
Science Monthly, Illustrated, for June, 1885.
Telegraphic Journal for June, 1885. 8vo.
Franklin Institute—Journal, No. 714. 8vo. 1885.
Geographical Society, Royal—Proceedings, New Series, Vol. VII. No. 6. 8vo. 1885.
Geological Institute, Imperial, Vienna—Abhandlungen, Band XI. Abtheilung 1. fol. 1885.
Jahrbuch: Band XXXV. Heft 1. 8vo. 1885.
Verhandlungen, 2–7. 8vo. 1885.
Jablonowski'sche Gesellschaft, Leipzig, Furstliche—Preisschrift, No. 25. 4to. 1885.
Johns Hopkins University—Studies in Historical and Political Science, Third Series, Nos. 5–7. 8vo. 1885.
American Journal of Philology, No. 21. 8vo. 1885.
Linnean Society—Journal, No. 108. 8vo. 1885.
Manchester Geological Society—Transactions, Vol. XVIII. Parts 8, 9. 8vo. 1885.
Medical and Chirurgical Society, Royal—Catalogue of the Library, Supplement III. 8vo. 1885.
Meteorological Office—Monthly Weather Report, February, 1885. 4to.
Meteorological Society, Royal—Quarterly Journal, No. 54. 8vo. 1885.
Meteorological Record, No. 16. 8vo. 1885.
Ministerio da Marinha e Ultramar—Cartas das Colonias Portuguezas: Angola. 1885.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXIV. Part 3. 8vo. 1885.
Oakley, R. Esq. (the Author)—Perfect Ventilation. fol. 1885.
Pennsylvania, Second Geological Survey of—Reports. 8vo. 1873–1884.
Pharmaceutical Society of Great Britain—Journal, June, 1885. 8vo.
Photographic Society—Journal, New Series, Vol. IX. No. 8. 8vo. 1885.
Physical Society of London—Proceedings, Vol. VI. Part 4. 8vo. 1885.
Robins, Edward C. Esq. F.S.A. (the Author)—Papers on Technical Education, Applied Science Buildings, Fittings, and Sanitation. 4to. 1885.
Society of Arts—Journal, June, 1885. 8vo.
Statistical Society—Journal, Vol. XLVIII. Part 1. 8vo. 1885.
Thom, Adam, Esq. LL.D. (the Author)—Emmanuel: the Scriptural Alphabets, and the Metallic Image. (Pentaglot.) 8vo. 1885.
United Service Institution, Royal—Journal, No. 129. 8vo. 1885.
United States Geological Survey—Copper-Bearing Rocks of Lake Superior. By R. D. Irving. 4to. 1883.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1885: Heft 5. 4to.
Wilson, W. E. Esq. M.R.I.—Thoughts on Science, Theology, and Ethics. By J. Wilson. 12mo. 1885.
Zoological Society—Proceedings, 1885, Part 1. 8vo. 1885.

GENERAL MONTHLY MEETING,

Monday, November 2, 1885.

WARREN DE LA RUE, Esq. M.A. D.C.L. F.R.S. Manager and Vice-President, in the Chair.

Joseph Wilson Swan, Esq.
Mrs. J. W. Swan,
General J. F. Tennant, R.E. F.R.S.

were elected Members of the Royal Institution.

The Special Thanks of the Members were offered to the President, His Grace The Duke of Northumberland, for his gracious offer to defray the cost of supplementing the electric lighting installation of the Institution by the requisite number of accumulators.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Palæontologia Indica: Series X. Vol. III. Part 6; Series XIII. Vol. I. Part 4, Fasc. 5; Series XIV. Vol. I. Part 3, Fasc. 5. 4to. 1885.
Memoirs, Vol. XXI. Parts 3-4. 8vo. 1885.
Records, Vol. XVIII. Part 3. 8vo. 1885.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quarta, Vol. V. Nos. 5-8. 8vo. And Disegni. fol. 1885.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. Vol. I. Fasc. 13-22. 8vo. 1884-5.
American Academy of Arts and Sciences—Memoirs, Vol. X. No. 3; Vol. XI. Part 2, No. 1. 4to. 1874-85.
Proceedings, Vol. XX. 8vo. 1885.
American Association for the Advancement of Science—Proceedings, Vol. V.-XVI. XXII.-XXVI. XXVIII.-XXXII. 8vo. 1851-85.
American Philosophical Society—Proceedings, Nos. 117-119. 8vo. 1884.
Amsterdam Royal Society of Zoology—Bijdragen tot de Dierkunde. Afl. 12. 4to. 1885.
Ashburner, Charles A. Esq. (the Author)—Anthracite Coal Fields of Pennsylvania. 8vo. 1884.
Publications of Second Geological Survey of Pennsylvania. 8vo. 1884.
Asiatic Society, Royal—Journal, Vol. XVII. Part 3. 8vo. 1885.
Asiatic Society of Bengal—Proceedings, Nos. 1-5. 8vo. 1885.
Journal, Vol. LIII. Part 2, No. 3; Vol. LIV. Part 1, Nos. 1, 2. 8vo. 1884.
Centenary Review, 1784 to 1883. 8vo. 1885.
Astronomical Society, Royal—Monthly Notices, Vol. XLV. No. 8. 8vo. 1885.
Memoirs, Vol. XLVIII. Part 2. 4to. 1884.
Australian Museum, Sydney—Catalogue of the Australian Hydroid Zoophytes. By W. M. Bale. 8vo. 1884.
Bankers, Institute of—Journal, Vol. VI. Part 7. 8vo. 1885.

Basel Naturforschende Gesellschaft—Verhandlungen, 7te Thiel, 3tes Heft. 1885.
Batavia Observatory—Rainfall in the East Indian Archipelago, 1884. 8vo. 1885.
Belgique Académie des Sciences, &c.—Mémoires, Tome XLV. 4to. 1884.
  Mémoires Couronnées, Tome XLVI. 4to. 1884. Tome XXXVI. 8vo. 1884.
  Bulletins, 3e Serie, Tomes VI. VII. VIII. 8vo. 1883-4.
  Annuaires, 1884 and 1885. 16to.
British Architects, Royal Institute of—Proceedings, 1884-5, No. 16; 1885-6, No. 1. 4to.
  Kalendar, 1885-6. 8vo.
  Transactions, New Series, Vol. I. 4to. 1885.
Chemical Society—Journal for July-Oct. 1885. 8vo.
Chief Signal Officer, U.S. Army—Professional Papers of the Signal Service, No. 15. 4to. 1884.
Civil Engineers' Institution—Minutes of Proceedings, Vol. LXXX. LXXXI. LXXXII. 8vo. 1885.
  List of Members. 8vo. 1885.
  Heat in its Mechanical Applications. Lectures, 1883-4. 8vo. 1885.
Cornwall Polytechnic Society, Royal—Fifty-second Annual Report. 8vo. 1884.
Corporation of the City of London—Calendar of Letters, 1350-1370. Edited by B. R. Sharpe. 8vo. 1885.
Crawford and Balcarres, The Earl of, F.R.S. M.R.I.—Dun Echt Observatory Publications: Vol. III. Mauritius Expedition, 1874, Division 2. 4to. 1885.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. V. Parts 4, 5. 8vo. 1885.
Dax: Société de Borda—Bulletins, 2e Serie, Dixième Année, Trimestre 3. 8vo. 1885.
Department of the Interior, U.S.—Land Laws of the United States. 3 vol. 8vo. 1884.
  The Public Domain. Its History, with Statistics. By T. Donaldson. 8vo. 1884.
Devonshire Association for the Advancement of Science, Literature, and Art—Report and Transactions, Vol. XVII. 8vo. 1885.
  The Devonshire Domesday, Part 2. 8vo. 1885.
Dunant, H. Esq. (the Author)—The Pyrophone. (M 9) 4to. 1885.
East India Association—Journal, Vol. XVII. No. 5. 8vo. 1885.
Editors—American Journal of Science for July-Oct. 1885. 8vo.
  Analyst for July-Oct. 1885. 8vo.
  Athenæum for July-Oct. 1885. 4to.
  Chemical News for July-Oct. 1885. 4to.
  Engineer for July-Oct. 1885. fol.
  Horological Journal for July-Oct. 1885. 8vo.
  Iron for July-Oct. 1885. 4to.
  Journal of Science for July-Oct. 1885. 8vo.
  Nature for July-Oct. 1885. 4to.
  Revue Scientifique for July-Oct. 1885. 4to.
  Science Monthly, Illustrated, for July-Oct. 1885. 8vo.
  Telegraphic Journal for July-Oct. 1885. 8vo.
Fatigati, Professor E. S. (the Author)—Reacciones quimicas en el campo del Microscopio. (O 19) (With Photographs) MS. 1885.
Franklin Institute—Journal, Nos. 715, 716, 717, 718. 8vo. 1885.
Fraser, Lieut.-Col. A. T. R.E. M.R.I. (the Author)—Darkness in the Land of Egypt. (K 107) 8vo. 1885.
Geographical Society, Royal—Proceedings, New Series, Vol. VII. Nos. 7, 8, 9, 10. 8vo. 1885.
Geological Institute, Imperial, Vienna—Jahrbuch: Band XXXV. Heft 2, 3. 8vo. 1885.
  Verhandlungen, 8, 9. 8vo. 1885.
Geological Society—Quarterly Journal, No. 163. 8vo. 1885.
Glasgow Philosophical Society—Proceedings, Vol. XVI. 8vo. 1884-5.

Gordon, Surgeon-General C. A. M.D. C.B. M.R.I. (the Author)—Analysis of Correspondence, &c. on Vivisection. (K 107) 8vo. 1885.

Gould, George, Esq. (the Author)—Shaksperian Corrigenda. (K 107) 8vo. 1884.

Greek Plays in relation to Dramatic Unities. (K 107) 8vo. 1883.

Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Tome XIX. Liv. 4, 5; Tome XX. Liv. 1, 2. 8vo. 1884–5.

Hirth, F. Esq. Ph.D. (the Author)—China and the Roman Orient: their Ancient and Mediæval Relations. 8vo. 1885.

Iron and Steel Institute—Journal for 1885, No. 1. 8vo.

Johns Hopkins University—Studies in Historical and Political Science, Third Series, Nos. 8, 9, 10. 8vo. 1885.

American Journal of Philology, No. 22. 8vo. 1885.

University Circular, Nos. 40, 41. 4to. 1885.

American Chemical Journal, Vol. VII. No. 2. 8vo. 1885.

Kramer, F. H. Esq. (the Author)—Die Radikalheilung der Diptheritis. 8vo. 1885.

Langley, S. P. Esq. (the Author)—The Temperature of the Surface of the Moon. (M 9) 4to. 1885.

Liveing, Professor G. D. M.A. F.R.S. (the Author)—Chemical Equilibrium the Result of the Dissipation of Energy. 12mo. 1885.

Linnean Society—Journal, Nos. 109, 138, 139. 8vo. 1885.

Lisbon, Sociedade de Geographie—Boletim, 4ª Serie, No. 12; 5ª Serie, Nos. 1, 2. 8vo. 1883.

Liverpool Literary and Philosophical Society—Proceedings, Vol. XXXVIII. 8vo. 1884.

Lubbock, Sir John, Bart. M.P. F.R.S. M.R.I. (the Author)—Representation. 8vo. 1885.

Madras Government—Telegraphic Longitude Determinations in India. 4to. 1884.

Madras Magnetical Observations, 1851–1855. 4to. 1884.

Singapore Magnetical Observations, 1841–5. 4to. 1851.

Manchester Geological Society—Transactions, Vol. XVIII. Part 10. 8vo. 1885.

Mechanical Engineers' Institution—Proceedings, Nos. 3, 4. 8vo. 1885.

Medical and Chirurgical Society, Royal—Proceedings, New Series, Vol. I. No. 10. 8vo. 1885.

Mensbrugghe, M. G. Van der (the Author)—Théorie Mécanique de la Tension Superficielle. (K 107) 8vo. 1885.

Meteorological Society, Royal—Quarterly Journal, No. 55. 8vo. 1885.

Meteorological Record, No. 17. 8vo. 1885.

Meteorological Office—Contributions to our Knowledge of the Meteorology of the Arctic Regions, Part 4. fol. 1885.

Quarterly Weather Report, 1877, Part 3. 4to. 1885.

Monthly Weather Report for March, April, 1885. 4to.

Hourly Readings, 1883, Parts 1, 2. 4to. 1885.

Middlesex Hospital—Reports for 1883. 8vo. 1885.

Montpellier Academie des Sciences et des Lettres—Mémoires, Tome X. Fasc. 3. 4to. 1884.

München, Sternwarte bei—Annalen, Supplementband, X. XIV. 8vo. 1871–84.

North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXIV. Parts 4, 5. 8vo. 1885.

Norwegischen Commission der Europaischen Gradmessung—Geodätische Arbeiten, Heft 4. 4to. 1885.

Vandstandsobservationer, Heft 3. 4to. 1885.

Numismatic Society—Chronicle and Journal, 1885, Part 2. 8vo.

Odontological Society of Great Britain—Transactions, Vol. XVII. No. 8. New Series. 8vo. 1885.

Pennsylvania, Second Geological Survey of—Reports. 8vo. 1874–1884.

Geological Hand Atlas of Pennsylvania. By J. P. Lesley. 8vo. 1885.

Perry, Rev. S. J. F.R.S. (the Author)—Results of Meteorological and Magnetical Observations, 1884. 12mo. 1885.

Pharmaceutical Society of Great Britain—Journal, July-Oct. 1885. 8vo.

Photographic Society—Journal, New Series, Vol. IX. No. 9; Vol. X. No. 1. 8vo. 1885.

Physical Society of London—Proceedings, Vol. VII. Parts 1, 2. 8vo. 1885.

Pogson, Miss E. Isis (the Reporter)—Report of the Meteorological Reporter to the Government of Madras, 1884–5. 8vo. 1885.

Pole, William, Esq. F.R.S. (the Author)—Further Data on Aerial Navigation. (Proc. Inst. Civil Engineers, Vol. 81.) 8vo. 1885.

Preussische Akademie der Wissenschaften—Sitzungsberichte, I.–XXXIX. 8vo. 1885.

Prince, C. Leeson, Esq. (the Author)—The Topography and Climate of Crowborough Hill, Sussex. [Privately Printed] 8vo. 1885.

Real y Pontificia Universidad de Sto. Tomas de Manila—Discurso por R. Arias. 8vo. 1885.

Richardson, B. W. M.D. F.R.S. (the Author)—The Asclepiad. Vol. II. Nos. 7, 8. 8vo. 1885.

Royal Society of London—Proceedings, Nos. 237, 238. 8vo. 1885.

Saunders, Laurence, Esq. (the Author)—Robert Boyle. A Biographical Sketch. (O 19) 12mo. 1885.

Saxon Society of Sciences, Royal—Philologisch-Historische Classe:
Abhandlungen: Band X. Nos. 1, 2. 4to. 1885.
Berichte, 1884 and 1885. 8vo.
Mathematische-Physische Classe:
Abhandlungen: Band XIII. Nos. 2, 3, 4. 4to. 1884–5.
Berichte, 1884 and 1885. 8vo.

Sibson, Mrs. M.R.I. (the Author)—Collected Works of Francis Sibson. Edited by W. M. Ord. 4 vols. 8vo. 1881.

Smithsonian Institution—Contributions to Knowledge, Vol. XXIV. XXV. 4to. 1885.

Society of Arts—Journal, July–Oct. 1885. 8vo.

Spratt, Vice-Admiral T. A. B. C.B. F.R.S. (the Author)—Report on Navigation of the River Mersey, 1884. 8vo. 1885.

Statistical Society—Journal, Vol. XLVIII. Parts 2, 3. 8vo. 1885.

St. Bartholomew's Hospital—Index to Reports, Vols. I.–XX. 8vo. 1885.

St. Pétersbourg, Académie des Sciences—Bulletin, Tome XXXI. No. 2. 4to. 1885.

Swedish Academy of Sciences, Royal—Handlingar (Mémoires), Band 18, 19. 4to. 1880–1.
Bihang (Supplément aux Mémoires), Band 6, 7, 8. 8vo. 1882–4.
Ofversigt (Bulletin), 1881, 1882, 1883.
Lefnadsteckningar (Biographies des Membres), Band 2, Heft 2. 8vo. 1883.

Telegraph Engineers, Society of—Journal, Vol. XIV. Nos. 57, 58. 8vo. 1885.

Teyler Museum—Archives, Série II. Vol. II. 2e Partie. 4to. 1885.

Tokio University—Memoirs, No. 5 (Appendix). 8vo. 1885.

Topley, William, Esq. F.G.S. (the Author)—The National Geological Surveys of Europe. (Brit. Assoc. Reports.) 8vo. 1885.

Trinity House—Report on Lighthouse Illuminants, Part 1. (P 14) fol. 1885.

United Service Institution, Royal—Journal, No. 130. 8vo. 1885.

United States Geological Survey—Monographs, Vol. VI.–VIII. 4to. 1883–4.
Bulletins, Nos. 2–6. 8vo. 1883–4.

Upsal University—Bulletin Mensuel de l'Observatoire de l'Université d'Upsal, Vol. XVI. 4to. 1884–5.
Nova Acta, Ser. III. Vol. XII. Fasc. 2. 4to. 1885.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1885: Heft 6, 7. 4to.

Victoria Institute—Journal, No. 74. 8vo. 1885.

Vincent, Benjamin, Esq. Assist. Sec. and Librarian R.I. (the Editor)—Haydn's Dictionary of Dates. 18th ed. 8vo. 1885.

Yorkshire Archæological and Topographical Association—Journal, Part 34. 8vo. 1885.

Zoological Society—Proceedings, 1885, Parts 2, 3. 8vo. 1885.
Transactions, Vol. XI. Part 10. 4to. 1885.

GENERAL MONTHLY MEETING,

Monday, December 7, 1885.

GEORGE BUSK, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

James Butcher, Esq.
Charles Bell Eustace Ford, Esq.

were elected Members of the Royal Institution.

The following Lecture Arrangements were announced:

PROFESSOR DEWAR, M.A. F.R.S. M.R.I. Fullerian Professor of Chemistry, R.I. Six Lectures (adapted to a Juvenile Auditory) on THE STORY OF A METEORITE. On Dec. 29 (*Tuesday*), Dec. 31, 1885; Jan. 2, 5, 7, 9, 1886.

ROBERT STAWELL BALL, Esq. LL.D. F.R.S. Andrews Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland. Three Lectures on THE ASTRONOMICAL THEORY OF THE GREAT ICE AGE. On *Tuesday*, Jan. 19, *Thursday*, Jan. 21, *Saturday*, Jan. 23.

REGINALD STUART POOLE, Esq. LL.D. of the British Museum, Corresp. Inst. France. Three Lectures on NAUCRATIS: (1) RELATIONS OF THE GREEKS WITH EGYPT FROM THE HEROIC AGE TO PSAMMETICHUS; (2) THE EMPORIUM OF NAUCRATIS; (3) THE EGYPTIAN SOURCES OF GREEK ART. On *Tuesdays*, Jan. 26, Feb. 2, 9.

CHARLES T. NEWTON, C.B. LL.D. M.A. Three Lectures on THE UNEXHIBITED PORTION OF THE GREEK AND ROMAN SCULPTURES IN THE BRITISH MUSEUM (illustrated by Drawings and Casts). On *Tuesdays*, Feb. 16, 23, March 2.

PROFESSOR ARTHUR GAMGEE, M.D. F.R.S. Fullerian Professor of Physiology, R.I. Six Lectures on THE FUNCTION OF CIRCULATION. On *Tuesdays*, March 9, 16, 23, 30, April 6, 13.

W. CHANDLER ROBERTS-AUSTEN, Esq. F.R.S. M.R.I. Chemist of the Mint. Four Lectures on METALS AS AFFECTED BY SMALL QUANTITIES OF IMPURITY. On *Thursdays*, Jan. 28, Feb. 4, 11, 18.

PROFESSOR W. BOYD DAWKINS, M.A. F.R.S. F.G.S. Four Lectures on THE ANCIENT GEOGRAPHY OF BRITAIN. On *Thursdays*, Feb. 25, March 4, 11, 18.

PROFESSOR TYNDALL, D.C.L. LL.D. F.R.S. M.R.I. Four Lectures on LIGHT. On *Thursdays*, March 25, April 1, 8, 15.

ARCHIBALD GEIKIE, Esq. LL.D. F.R.S. Director-General of the Geological Survey of the United Kingdom. Four Lectures on THE HISTORY OF VOLCANIC ACTION IN THE BRITISH ISLES. On *Saturdays*, Jan. 30, Feb. 6, 13, 20.

REV. C. TAYLOR, D.D. Master of St. John's College, Cambridge. Two Lectures on THE HISTORY OF GEOMETRY: THE GREEKS AND THE MODERNS. On *Saturdays*, Feb. 27, March 6.

EDWARD B. POULTON, Esq. M.A. Two Lectures on THE NATURE AND PROTECTIVE USE OF COLOUR IN CATERPILLARS. On *Saturdays*, March 13, 20.

HOWARD GRUBB, Esq. F.R.S. Two Lectures on THE ASTRONOMICAL TELESCOPE. On *Saturdays*, March 27, April 3.

PROFESSOR OLIVER LODGE, D.Sc. Two Lectures on FUEL AND SMOKE. On *Saturdays*, April 10, 17.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Nautical Almanac for 1889. 8vo. 1885.
  Greenwich Observations for 1883. 4to. 1885.
  Greenwich Spectroscopic and Photographic Results for 1883. 4to. 1885.
  Cape Catalogue of Stars for 1850. 8vo. 1883.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quarta, Vol. V. No. 9. 8vo. And Disegni. fol. 1885.
Abel, Sir Frederick, C.B. D.C.L. F.R.S. M.R.I. (the Author)—Address to the Society of Arts (Explosions in Coal Mines). 8vo. 1885.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. Vol. I. Fasc. 23, 24. 8vo. 1884–5.
Academy of Natural Sciences, Philadelphia—Proceedings, 1885, Part 2. 8vo.
Agricultural Society of England, Royal—Journal, Second Series, Vol. XXI. Part 2. 8vo. 1885.
  General Index to Second Series of Journal, Vols. XI.–XX. 8vo. 1885.
Antiquaries, Society of—Proceedings, Vol. X. No. 2. 8vo. 1885.
Astronomical Society, Royal—Monthly Notices, Vol. XLV. No. 9, Sup. 8vo. 1885.
Bankers, Institute of—Journal, Vol. VI. Part 8. 8vo. 1885.
Birmingham Philosophical Society—Proceedings, Vol. IV. Part 2. 8vo. 1884–5.
Birt, William, Esq.—Poppy Land. By Clement Scott. 12mo. 1885.
British Architects, Royal Institute of—Proceedings, 1885–6, Nos. 2, 3. 4to.
Cambridge Philosophical Society—Proceedings, Vol. V. Part 4. 8vo. 1885.
Chapman, Henry, Esq. M.Inst.C.E. (the Author)—Compound Locomotives. fol. 1885.
Chemical Society—Journal for November, 1885. 8vo.
Civil Engineers' Institution—Theory and Practice of Hydro-Mechanics (Lectures). 8vo. 1885.
Editors—American Journal of Science for November, 1885. 8vo.
  Analyst for November, 1885. 8vo.
  Athenæum for November, 1885. 4to.
  Chemical News for November, 1885. 4to.
  Engineer for November, 1885. fol.
  Horological Journal for November, 1885. 8vo.
  Iron for November, 1885. 4to.
  Nature for November, 1885. 4to.
  Revue Scientifique for November, 1885.
  Science Monthly, Illustrated, for November, 1885.
  Telegraphic Journal for November, 1885. 8vo.
Franklin Institute—Journal, No. 719. 8vo. 1885.
General Medical Council—Report of Statistical Committee. 8vo. 1885.
Geographical Society, Royal—Proceedings, New Series, Vol. VII. No. 11. 8vo. 1885.
Geological Society—Quarterly Journal, No. 164. 8vo. 1885.
Johns Hopkins University—University Circular, Nos. 42–44. 4to. 1885.
  American Chemical Journal, Vol. VII. No. 3. 8vo. 1885.
Kew Observatory—History. By R. H. Scott. (Proc. Royal Soc.) 1885.
Lisbon, Sociedade de Geographie—Boletim, 5ª Serie, Nos. 3–5. 8vo. 1885.
Madrid Literary and Scientific Athenæum—La Cuestion Ferran en el Ateneo. 12mo. 1885.
Manchester Geological Society—Transactions, Vol. XVIII. Part 11. 8vo. 1885.

Mechanical Engineers' Institution—Proceedings, Index, 1874–1884. 8vo. 1885.
Melbourne Public Library—Map of Early Melbourne. By R. Russell. 1885.
Meteorological Office—Monthly Weather Report, May, 1885. 4to.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXIV. Part 6. 8vo. 1885.
Pharmaceutical Society of Great Britain—Journal, November, 1885. 8vo.
Photographic Society—Journal, New Series, Vol. X. No. 2. 8vo. 1885.
Royal College of Surgeons of England—Calendar. 8vo. 1885.
Royal Society of New South Wales—Journal and Proceedings, Vol. XVIII. 8vo. 1884–5.
Royal Society of Tasmania—Catalogue of the Library. 8vo. 1885.
Smithsonian Institution—Report, 1883. 8vo. 1885.
Society of Arts—Journal, November, 1885. 8vo.
Startin, James, Esq. (*the Author*)—Syphilitic Eruptions of the Skin. 8vo. 1885.
  Lecture on a Healthy Skin. 8vo. 1885.
  Lecture on the Diseases of the Skin and Hair. 12mo. 1885.
St. Pétersbourg, Academie des Sciences—Mémoires, Tome XXXII. Nos. 14–18; Tome XXXIII. Nos. 1, 2. 4to. 1885.
United Service Institution, Royal—Journal, No. 131. 8vo. 1885.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1885: Heft 8. 4to.

NOTES RELATING TO PROFESSOR DEWAR'S LECTURES

ON THE STORY OF A METEORITE.

Delivered on December 29 and 31, 1885, and January 2, 5, 7, 9, 1886.

ACCOUNT OF THE DHURMSALA METEORITE.

Report to Punjab Government, dated Dhurmsala, 28 *July,* 1860.

ON the afternoon of Saturday, the 14th of July, 1860, between the hours of 2 and 2.30 P.M., the station of Dhurmsala was startled by a terrific bursting noise, which was supposed at first to proceed from a succession of loud blastings or from the explosion of a mine in the upper part of the station; others, imagining it to be an earthquake or very large landslip, rushed from their houses in the firm belief that they must fall upon them.

It soon became apparent that this was not the case. The first report, which was far louder in its discharge than any volley of artillery, was quickly followed by another and another, to the number of fourteen or sixteen. Most of the latter reports grew gradually less and less loud. These were probably but the reverberations of the former, not among the hills but amongst the clouds, just as is the case with thunder. It was difficult to say which were the reports and which the echoes. There could certainly not have been fewer than four or five actual reports. During the time that the sound lasted the ground trembled and shook convulsively.

From the different accounts of three eye-witnesses there appears to have been observed a flame of fire, described as about 2 feet in depth and 9 feet in length, darting in an oblique direction above the station after the first explosion had taken place.

The stones as they fell buried themselves from a foot to a foot and a half in the ground, sending up a cloud of dust in all directions.

Most providentially no loss of life or property has occurred.

Some coolies, passing by where one fell, ran to the spot to pick up the pieces; before they had held them in their hands half a minute they had to drop them owing to the intensity of the cold which benumbed their fingers.

This, considering the fact that they were apparently but a moment before in a state of ignition, is very remarkable. Each stone that fell bore unmistakable marks of partial fusion,

The morning and afternoon preceding the occurrence had been particularly dull and cloudy. Temperature was close, sultry, and oppressive. The thermometer was above 80° of Fahrenheit, and no rain had fallen. I had no barometer by me at the time; I am therefore unable to state what was the precise pressure of the atmosphere.

The clouds, which were of the form technically called cumulus and cirrhus, were hanging low at the time, and the atmosphere heavily charged with electricity.

Such are simply the facts of the case as they occurred.

There are of course all sorts of conjectures as to the probable cause of the occurrence. Some state the stones to be of volcanic origin; others that they were hurled from the heights above the station or projected from the moon; but I am inclined to regard them as real *boná-fide* meteorolites. Their weight seems to indicate that they are semi-metallic substances, composed probably of meteoric iron alloyed with nickel and mixed with silica and magnesia or some other earthy substance. They are nearly double the weight of a piece of ordinary stone of similar dimensions.

Another very singular phenomenon was witnessed at Dhurmsala on the evening of the same day that the aerolite fell. This appears to have been a succession of igneous meteors, such as fire-balls, or falling or shooting stars. This singular sight did not attract the attention of most people. I quote the account from the writer who describes it *verbatim.*

"I think it was on the evening of the same day that the meteor fell that I observed lights in the air. They commenced to appear about 7 P.M., and lasted for about three hours till 10; they appeared for about one minute, some for longer, then went out again, other lights appearing in their places; sometimes three or four lights appeared in the same place together, and one or two moved off, the others remaining stationary, they looked like fire-balloons, but appeared in places where it was impossible for there to have been any houses or any roads, where people could have been. Some were high up in the air moving like fire-balloons, but the greater part of them were in the distance, in the direction of the lower hills, in front of my house, others closer to our house, and between Sir A. Lawrence's and the Barracks. I am sure from some which I observed closely that they were neither fire-balloons, lanterns, nor bonfires, or any other thing of that sort, but *boná-fide* lights in the heavens. Though I made enquiries amongst the natives the next day, I have never been able to find out what they were or the cause of their appearance."

Another Account.

About 2 P.M. on Saturday, the 14th of July, a tremendous mid-air explosion was heard at Dhurmsala, Kangra, Dalhousie, Madhoopoor and Goordaspoor. The vapour or smoke following the explosion was distinctly *seen* at Dalhousie about thirty miles, and at Kangra ten miles from Dhurmsala, where the explosion, said to have resembled the discharge of an 84-pounder, was followed by the descent in various parts of the station, some two miles apart, of large masses of aerolite. One piece that fell near the Dhurmsala

Police Battalion Lines, was ascertained to have been when entire, one foot in diameter, but it was broken into several fragments.

I was at the time, reading with my Moonshi in my study, and heard an extraordinary noise like that of thunder at a short distance. There could be no doubt that it was near, and I immediately supposed it was something else than thunder. The steady rattling noise which appeared to be travelling in a horizontal direction gradually increased to one tremendous majestic clap; after which the former steady rattling noise continued perhaps for a minute, till at last it died off very gradually. The noise appeared to be so low that I thought a volcano or something like it would immediately appear somewhere in our valley. A servant of mine happened just to return from the Post Office, and told me that above the hill on which our house is situate he had seen a fire travelling towards Dhurmsala, till at last it disappeared. The sky was cloudy, yet there were no such clouds as would justify the opinion that lightning and thunder had issued from them.

Chemical Analysis of a Meteoric Stone from Dhurmsala.

By Dr. C. T. Jackson.

The most curious fact alleged in the report is, that the pieces, which were picked up immediately after they fell, when held in the hand for half-a-minute, were so cold as to benumb the fingers, and this is mentioned as very remarkable, since a few moments before the surface of the meteorite was in a state of ignition, and still bears evident marks of partial fusion.

The temperature of the day was 80° F., and the cold could not have been occasioned by the soil in that climate. Indeed, the temperature required to produce the effect alleged must have been far below zero.

Now, supposing the fact to be true, that it was intense cold that was produced by the stone, may it not have been owing to the low temperature of the region from which the meteorite fell? the interplanetary spaces, according to Baron Fourier's estimate, being about $-50°$ Centigrade, or nearly 100° Fahr. below freezing point.

Allowing that the meteoric mass came from those regions, the matter being a very slow conductor of heat, we can easily conceive that when the mass entered the earth's atmosphere, it might become heated and inflamed on the surface by condensing the air before it, in its descent towards the earth; and since it would have to fall through about eighty miles of the atmosphere, the density of which increases as it approaches the earth, the inflammation would take place only where the air had sufficient density, and not in the highest regions. Such being the case, the expansion of the exterior of the meteorite, the surface being incandescent, while the interior was very cold, would cause the mass to fly to pieces with violent detonations, and this, too, quite near to the earth.

The surface of so imperfect a conductor of heat might be ignited, while the interior of the mass remained intensely cold. We know that imperfect conductors of heat, when heated to redness, and plunged into cold water, so that they can be momentarily handled, will again become nearly red hot on the surface, by heat derived from the interior. Thus, specimens of lavas, which I collected in the crater of Vesuvius, handled freely, and wrapped up in paper, frequently set fire to the paper in a short time after they were so enveloped. I brought home many specimens which had browned and charred the paper.

It is also known to all assayers and chemists, that a crucible full of melted flux, if cooled on the surface by plunging the crucible into water, will soon become hot again on the surface, and that the interior of the flux will remain red hot, while the surface of the crucible may be held in the hand for a short time.

Therefore, *mutatis mutandis*, there is no inherent improbability that these masses of meteoric stone really would produce the sensation of intense cold, if they were originally cold in the interior, and only rapidly heated on the surface. If the facts are as alleged, this is the first recorded recognition by the human senses of the cold of the interplanetary regions. It would have been a curious and instructive experiment, to have placed one of these stones, soon after it fell, in water, when the formation of a crust of ice on the surface would have visibly demonstrated the fact of intense cold; and an estimate of the degree of cold could also have been made, by similar means, ascertaining how much the temperature of a given quantity of water was reduced by it, and computing the degrees of cold thereby.

The weight of the fragment presented to the Society is 4½ ounces. It is 2½ inches long, 1¼ inches wide, and 1 inch in average thickness.

Its specific gravity is 3·456 at 68° Fahr., Barom. 29·9. Its structure is imperfectly granular, but not crystallised, and there are small black specks of the size of a pin's head, and smaller, of malleable meteoric iron, which is readily removed from the crushed stone by the magnet. The colour of the mass is ash grey. A portion of the surface is black and is scorified by fusion.

Its hardness is not superior to that of olivine or massive chrysolite. Chemical analysis shows that its composition is that of a ferruginous olivine.

One gramme of the stone, crushed in an agate mortar, and acted on by a magnet, yielded 0·43 grm. of meteoric iron, which was malleable. After the removal of this a qualitative analysis was made of the residual powder. Another gramme was also taken, without picking out the metallic iron, and was tested for chlorine and for phosphoric acid. The results of the qualitative analysis were that the stone contains silica, magnesia, a little alumina, oxide of iron and nickel, a little tin, an alloy of iron and nickel, phosphoric acid, and a trace of chlorine.

These ingredients being determined, the plan for a quantitative analysis was laid out, and was duly executed by the usual and approved methods. The following are the results of this analysis, per centum:

Silica, with traces of tin	40·000
Magnesia	26·600
Per-oxide of iron	27·700
Metallic iron	3·500
Metallic nickel	0·800
Alumina	0·400
Chlorine	0·049
Phosphoric acid .. .. not weighed	—
	99·049

Analysis of Gases in Meteorite (Ansdell and Dewar).

Carbonic acid	61·29
Carbonic oxide	7·52
Hydrogen	30·96
Nitrogen	0·23
	100·00

The meteorite contains about three times its volume of gas.

Chladni's Theory (*Nichol*).

"This theory, first proposed by Chladni in 1794, may be best put in the following general form:—

Through the interplanetary spaces, and it may be, through the interstellar spaces also, vast numbers of small masses of solid matter may be moving in irregular orbits; and these, as they approach any planet of powerful gravitation—such as the earth—will be disturbed, and may fall towards its surface.

Chladni's hypothesis certainly explained much, but one essential part of the phenomenon it did not explain.

For instance, it was quite consistent with the fact that some of these bodies fall to the earth as aerolites, and that others escape as mere falling stars; but Chladni could, in his day, give no account whatever of the heat of the stones that do fall, and the apparent inflammation of those that only pass through our atmosphere, and appear as falling stars.

The desideratum, however, has been supplied by modern physics.

No compression of the atmosphere certainly, by any body moving through it, could evolve heat enough to produce such results; but the recent and apparently established conception regarding heat, viz. that it must be evolved as an equivalent for any destroyed mechanical effect, wholly removes the difficulty.

The velocity of a sufficient number of these falling stars, for instance, has been ascertained within due limits by Brandes and others; and M. Joule has shown conclusively, that in regard of the greater number of these bodies—the heat, equivalent to the mechanical effect due to their original vis viva, and destroyed by the resistance of the atmosphere—is such as would melt the body and dissipate it into fragments.

In case of smaller velocities, nothing beyond inflammation or white heat might ensue; but far oftener than we imagine, these falling stars are utterly dissipated by the agency now spoken of, and reach the earth in the form of mere meteoric dust.

This special difficulty removed from Chladni's cosmical theory, other problems remained. First, Have these stones, or meteoric planets, a special or assignable origin?

The lunar hypothesis of Laplace has frequently found favour. But it ought not to be judged by the form in which it was proposed by its founder.

The idea that these meteors are directly shot towards us by lunar volcanoes in present action, is consistent neither with existing observation of the condition of the moon, nor with the dynamical essentialia of the problem.

But it does not follow that those vast lunar craters—such as Tycho—the result evidently of enormous cataclysms, have not contributed their part in driving among the interplanetary spaces, masses of broken rock, that may on occasion come within the range of the special attraction of our globe. But secondly, is it necessary to search for any confined or special origin? Is it not manifest, on the contrary, that masses of such bodies are most widely diffused, and may form an essential part—not of our solar system merely—but of the material universe, in so far as man can discern it?"

Joule's Explanation of the Heating of Meteorites.

"Behold, then, the wonderful arrangements of Creation. The earth, in its rapid motion round the sun, possesses a degree of living force so vast that, if turned into the equivalent of heat, its temperature would be rendered at least 1000 times greater than that of red-hot iron, and the globe on which we tread would in all probability be rendered equal in brightness to the sun itself.

And it cannot be doubted that if the course of the earth were changed, so that it might fall into the sun, that body, so far from being cooled down by the contact of a comparatively cold body, would actually blaze more brightly than before, in consequence of the living force with which the earth struck the sun being converted into its equivalent of heat. Here we see that our existence depends upon the maintenance of the living force of the earth.

On the other hand, our safety equally depends in some instances upon the conversion of living force into heat.

You have, no doubt, frequently observed what are called shooting stars, as they appear to emerge from the dark sky of night, pursue a short and rapid course, burst, and are dissipated in shining fragments.

From the velocity with which these bodies travel, there can be little doubt that they are small planets which, in the course of their revolution round the sun, are attracted and drawn to the earth.

Reflect for a moment on the consequences which would ensue, if a hard meteoric stone were to strike the room in which we are assembled with a velocity sixty times as great as that of a cannon-ball.

The dire effects of such a collision are effectually prevented by the atmosphere surrounding our globe, by which the velocity of the meteoric stone is checked, and its living force converted into heat, which at last becomes so intense as to melt the body and dissipate it into fragments too small probably to be noticed in their fall to the ground.

Hence it is that, although multitudes of shooting stars appear every night, few meteoric stones have been found, those few corroborating the truth of our hypothesis by the marks of intense heat which they bear on their surface.

The average result of the experiments of Joule and Thomson showed that the wire was warmed 1° Cent. by moving at the velocity of 175 feet per second.

The highest velocity obtained was 372 feet per second, which gave a rise of 5°·3 Cent.; and there was no reason to doubt that the thermal effect would go on continually increasing with the square of the velocity. Thus at a mile per second the rise of temperature would be, in round numbers, 900° Cent.; and at twenty miles per second, which may be taken as the average velocity with which meteorites strike the atmosphere of the earth, 360,000°.

The temperature due to the stoppage of air at the velocity of 143 feet per second is 1° Cent. Hence we may infer that the rise observed in the experiments was that due to the stoppage of air, less a certain quantity, of which probably the greater part is owing to loss by radiation. It being also clear that the effect is independent of the density of the air, there remains no doubt as to the real nature of 'shooting stars.'

These are small bodies which come into the earth's atmosphere at velocities of twenty miles per second and upwards.

The instant they touch the atmosphere their surfaces become heated far beyond the point of fusion—even of volatilisation—and the consequence is that they are speedily and, for the most part, completely burnt down and reduced to impalpable oxides. It is thus that, by the seemingly feeble resistance of the atmosphere, Providence secures us effectively from a bombardment which would destroy all animated nature exposed to its influence."

[J. D.]

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 22, 1886.

Sir William Bowman, Bart. LL.D. F.R.S. Vice-President, in the Chair.

Professor Tyndall, D.C.L. LL.D. F.R.S. *M.R.I.*

Thomas Young and the Wave Theory.

(Abstract deferred.)

WEEKLY EVENING MEETING,

Friday, January 29, 1886.

William Huggins, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

Sir William Thomson, D.C.L. LL.D. F.R.S.

Capillary Attraction.

(Abstract deferred.)

GENERAL MONTHLY MEETING,

Monday, February 1, 1886.

Warren de la Rue, Esq. M.A. D.C.L. F.R.S. Vice-President, in the Chair.

William Anderson, Esq. M. Inst. C.E.
Henry Arthur Blyth, Esq.
James Blyth, Esq.
James Crowdy, Esq.
Brownlow D. Knox, Esq.
Mrs. Shepherd,
W. F. R. Weldon, Esq. M.A.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mr. JAMES WIMSHURST, *M.R.I.* for his valuable present of two Electrical Influence Machines.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Records, Vol. XVIII. Part 4. 8vo. 1885.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quarta, Vol. V. Nos. 10, 11. 8vo. And Disegni. fol. 1885.
New Zealand Government—Statistics of the Colony of New Zealand for 1884. fol. 1885.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. Vol. I. Fasc. 25, 26, 27, 28. 8vo. 1885.
American Philosophical Society—Proceedings, No. 120. 8vo. 1885.
Asiatic Society, Royal—Journal, Vol. XVIII. Part 1. 8vo. 1885.
Asiatic Society of Bengal—Proceedings, Nos. 6–8. 8vo. 1885.
Journal, Vol. LIV. Part II. Nos. 1, 2. 8vo. 1885.
Astronomical Society, Royal—Monthly Notices, Vol. XLVI. Nos. 1, 2. 8vo. 1885.
Australian Museum, Sydney—Report of the Trustees, 1885. fol.
Bankers, Institute of—Journal, Vol. VI. Part 9; Vol. VII. Part 1. 8vo. 1885–6.
Batavia Observatory—Magnetical and Meteorological Observations, Vol. VI. fol. 1885.
Boston Society of Natural History—Memoirs, Vol. III. No. 11. 4to. 1885.
Proceedings, Vol. XXII. Part 4; Vol. XXIII. Part 1. 8vo. 1883–4.
British Architects, Royal Institute of—Proceedings, 1885–6, Nos. 4, 5, 6, 7. 4to.
British Museum (Natural History)—Catalogue of Lizards. 2nd edition. Vol. 2. 8vo. 1885.
Chemical Society—Journal for Dec. 1885 and Jan. 1886. 8vo.
Clinical Society—Transactions, Vol. XVIII. 8vo. 1885.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. V. Part 6. 8vo. 1885.
Dawson, Dr. G. M. F.G.S. (the Author)—Boulder Clays. 8vo. 1885.
Dax: Société de Borda—Bulletins, 2ᵉ Serie, Dixième Année, Trimestre 4. 8vo. 1885.
East India Association—Journal, Vol. XVIII. No. 1. 8vo. 1886.
Editors—American Journal of Science for Dec. 1885 and Jan. 1886. 8vo.
Analyst for Dec. 1885 and Jan. 1886. 8vo.
Athenæum for Dec. 1885 and Jan. 1886. 4to.
Chemical News for Dec. 1885 and Jan. 1886. 4to.
Engineer for Dec. 1885 and Jan. 1886. fol.
Horological Journal for Dec. 1885 and Jan. 1886. 8vo.
Iron for Dec. 1885 and Jan. 1886. 4to.
Nature for Dec. 1885 and Jan. 1886. 4to.
Revue Scientifique for Dec. 1885 and Jan. 1886. 4to.
Science Monthly, Illustrated, for Dec. 1885 and Jan. 1886. 8vo.
Telegraphic Journal for Dec. 1885 and Jan. 1886. 8vo.
Franklin Institute—Journal, Nos. 720, 721. 8vo. 1885–6.
Geneva: Société de Physique et d'Histoire Naturelle—Mémoires, Tome XXIX. Partie 1. 4to. 1884–5.
Geographical Society, Royal—Proceedings, New Series, Vol. VII. No. 12; Vol. VIII. No. 1. 8vo. 1885.
Supplementary Papers, Vol. I. Parts 2, 3. 8vo. 1884–5.
Georgofili, Reale Accademia—Atti, Quarta Serie. Vol. VIII. Disp. 2, 3. 8vo. 1885.
Gordon, Surgeon-General C. A. C.B. M.D. M.R.I.—Gospel of St. Matthew. Rifian Translation by W. Mackintosh. 12mo. 1885.

Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Tome XX. Liv. 3. 8vo. 1885.

Haviland, Alfred, Esq. (the Author)—Consumption. 8vo. 1885.
The Isle of Man and the Glacial Period. 8vo. 1886.

Historical Society, Royal—Transactions, New Series, Vols. I. II. III. Part 1. 8vo. 1883-5.

Johns Hopkins University—Studies in Historical and Political Science, Third Series, Nos. 11, 12. 8vo. 1885.
American Journal of Philology, No. 23. 8vo. 1885.
University Circular, No. 45. 4to. 1885.
American Chemical Journal, Vol. VII. Nos. 4, 5. 8vo. 1885.

Linnean Society—Journal, Nos. 112, 140, 141. 8vo. 1885.

Lisbon, Sociedade de Geographie—Boletim, 5a Serie, No. 6. 8vo. 1885.

Lynd, William, Esq. (the Author)—Practical Telegraphist. 12mo. 1885.
The Telegraphist. 4to. 1884-5.

Manchester Geological Society—Transactions, Vol. XVIII. Parts 12-13. 8vo. 1885-6.

Manchester Literary and Philosophical Society—Memoirs, 3rd Series, Vol. VIII. 8vo. 1884.
Proceedings, Vols. XXIII. and XXIV. 8vo. 1884-5.

Mathieson and Co. Messrs. F. C.—Vade Mecum for Investors for 1886. 12mo.

Mechanical Engineers' Institution—Proceedings, No. 5. 8vo. 1885.

Medical and Chirurgical Society, Royal—Proceedings, New Series, Vol. II. No. 1. 8vo. 1885.
Transactions, Vol. LXVIII. 8vo. 1885.

Meteorological Society, Royal—Quarterly Journal, No. 56. 8vo. 1885.
Meteorological Record, No. 18. 8vo. 1885.

Meteorological Office—Monthly Weather Report for June, July, August, 1885. 4to.

Numismatic Society—Chronicle and Journal, 1885, Part 3. 8vo.

Odontological Society of Great Britain—Transactions, Vol. XVIII. No. I. New Series. 8vo. 1885.

Pharmaceutical Society of Great Britain—Journal, Dec. 1885 and Jan. 1886. 8vo.
Calendar for 1886. 8vo.

Photographic Society—Journal, New Series, Vol. X. No. 3. 8vo. 1885.

Physical Society of London—Proceedings, Vol. VII. Part 3. 8vo. 1886.

Radcliffe Observatory—Radcliffe Observations for 1882. 8vo. 1885.

Royal Society of Canada—Proceedings and Transactions, Vol. II. (1884). 8vo. 1885.

Royal Society of Edinburgh—Proceedings, Nos. 115-120. 8vo. 1883-5.

Royal Society of London—Proceedings, No. 239. 8vo. 1885.

Sanitary Institute of Great Britain—Transactions, Vol. VI. 8vo. 1885.

Seismological Society of Japan—Transactions, Vol. VIII. 8vo. 1885.

Smith, Willoughby, Esq. M.R.I. (the Author)—Magnetism. 8vo. 1885.

Society of Arts—Journal, Dec. 1885 and Jan. 1886. 8vo.

Society for Psychical Research—Proceedings, Part 9. 8vo. 1885.

Telegraph Engineers, Society of—Journal, Vol. XIV. No. 59. 8vo. 1885.

Teyler Museum—Archives, Série II. Vol. II. 3e Partie. 4to. 1885.
Catalogue de la Bibliothéque. Par C. Ekama. Liv. 1, 2. 8vo. 1885.

Tokio University—Memoirs, No. 12. 8vo. 1885.

Trinity House—Report on Lighthouse Illuminants, Part 2. fol. 1885.

United States Geological Survey—Fourth Annual Report, 1882-3. 4to. 1884.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1885: Heft 9, 10. 4to.

Victoria Institute—Journal, No. 75. 8vo. 1885.

Yorkshire Archæological and Topographical Association—Journal, Part 35. 8vo. 1886.

WEEKLY EVENING MEETING,

Friday, February 5, 1836.

Sir Frederick Bramwell, LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

T. Pridgin Teale, Esq. M.A. F.R.C.S.

The Principles of Domestic Fireplace Construction.

If there be a place in the kingdom in which a lecture on the subject selected for to-night could appropriately be given, surely it is the theatre in which we are assembled. Some of my hearers may be aware of the mutual fitness of subject and place. Many, perhaps, are not aware, as, indeed, was the case with myself three months ago, that the principles of fireplace construction which will be laid before you to-night, and which I have been working out and teaching for the last three or four years, were urged, written about, and acted upon at the end of the last century by your Founder, Count Rumford, and that a great portion of his time, his writings, and his work was devoted to this very question.

Hardly any subject would be more in harmony with the aims which he set before him in founding this Society, as we may learn from the following quotation from the "Prospectus of the Royal Institution," published at the end of the 5th volume of Rumford's Works:—"But if it should be proved, as in fact it may, that in the applications of fire, in the management of heat, and in the production of light, we do not derive half the advantage from combustion which might be obtained, it will readily be admitted that these subjects must constitute a very important part of the useful information to be conveyed in the public lectures of the Royal Institution." (V. p. 784.)

And why should it be necessary, at the end of this nineteenth century, to give a lecture on "The principles of fireplace construction"? Why should such a title draw together an audience? Clearly from the fact that correct principles have been habitually, and, until the last few years, almost universally violated, and because the rules so ably worked out, so earnestly and forcibly advocated by Rumford, have lain dormant, lingering here and there, chiefly in old-fashioned houses, and almost forgotten.

Again, why should a layman, whose profession lies outside that of the architect, the builder, and the manufacturer, take upon himself to teach principles that are to guide other professions than his own?

Mainly for two reasons: one, that there are principles which a medical man may work out without reproach, as tending to contribute to the happiness, the comfort, and the health of mankind; the other, that when principles have to be insisted upon, and to be made a subject of public instruction, they can be urged with more effect by those who are hampered by no relations to any patents, and have no pecuniary interest in the success or failure of the application of the principles in question. On this point we have a good example in Count Rumford, who says in a note:—"The public in general and particularly those tradesmen and manufacturers whom it may concern, are requested to observe, that, as the author does not intend to take out any patent for any invention of his which may be of public utility, all persons are at full liberty to imitate them, and vend them for their own emolument, when, and where, and in any way they may think proper." (III. 527.)

Three evils result from the prevalence of bad principles in construction:—1. Waste of fuel and loss of heat. 2. Excessive production of soot and smoke. 3. Large addition to ashpit refuse by cinders, which are really unburnt, and therefore wasted fuel. These are matters of national concern, and it has been the main object of my labours on this question during the last four years to endeavour to convince the public that it is the interest no less than the duty of every householder, to burn his fuel on correct principles, and to do his part towards the diminution of these evils.

On the first point, "Waste of fuel and heat," let us listen to Rumford, whose words are as true to-day as when written eighty years ago. "Though it is generally acknowledged that there is a great waste of fuel in all countries, arising from ignorance and carelessness in the management of fire, yet few—very few, I believe—are aware of the real amount of this waste." (IV. 5.) "From the result of all my inquiries upon this subject, I have been led to conclude that not less than *seven-eighths* of the heat generated, or which *with proper management might be generated*, from the fuel actually consumed, is carried up into the atmosphere with the smoke, and totally lost." (IV. 6.) "And with regard to the economy of fuel, it has this in particular to recommend it, that whatever is saved by an individual is at the same time a positive saving to the whole community." (IV. 4.)

Heat is wasted in three ways—either by combustion under the impulse of strong draught, which means rapid escape of heat up the chimney; or by imperfect combustion of the gases which are generated during the burning of the coals; or by escape of heat through the iron sides and back into the space between the range and the brickwork, and so into the chimney. The greatest offenders are the ordinary register grates. Iron all over, back, and sides, and roof, they are usually set in a chamber open above to the chimney, and imperfectly filled in, or not filled in at all, with brickwork. The heat escapes through the iron to this chamber, and thence is lost. Another fault is that the "register opening," in other words the "throat of the chimney," being immediately above the coal, submits the burning fuel to the full concentrated force of the current to the chimney, converting the fire into a miniature blast-furnace. On this point Rumford says:—"But there are, I am told, persons in this country who are so fond of seeing what is called a great roaring fire, that even with its attendant inconveniences, of roasting and freezing opposite sides of the body at the same time, they prefer it to the genial and equable warmth which a smaller fire, properly managed, may be made to produce, even in an open chimney fireplace." (III. 569.)

The second result of faulty construction in fireplaces is "Undue production of smoke and soot." Smoke and soot imply imperfect combustion, and to this two defects in a fire mainly contribute, one, too rapid a draught through the fire which hurries away and chills below burning point the gas rising from the heated fuel. The other defect is too cold a fire, i. e. too small a body of heat in and around the fuel, so that the temperature of the gases is not raised to a point at which they will burn. On the smoke question Rumford waxes eloquent:—"The enormous waste of fuel in London may be estimated by the vast dark cloud which continually hangs over this great metropolis, and frequently overshadows the whole country, far and wide; for this dense cloud is certainly composed almost entirely of *unconsumed coal*, which, having stolen wings from the innumerable fires of this great city, has escaped by the chimneys, and continues to sail about in the air till, having lost the heat which gave it volatility, it falls in a dry shower of extremely fine black dust to the ground, obscuring the atmosphere in its descent, and frequently changing the brightest day into more than Egyptian darkness."

A few years ago the prevalence of unusually dense fogs roused the metropolitan public to a sense of this great evil. The Smoke Abatement Society was formed, and under its auspices exhibitions of smoke-consuming apparatus and improved fireplaces were held in London and Manchester. Beyond the fact that certain grates were pronounced to be good in point of economy, and moderate in the production of smoke, and that the public has been led to take an interest in and enquire into the relative value and economy of various patent fireplaces, there has been but little advance in the education of the public in the principles which lie at the root of the whole question.

A third result of bad construction is the "Production of cinders." With good coal, cinders are inexcusable. They are unconsumed carbon—coke—and imply a faulty fireplace. If thrown into the ashpit, as is the case in 99 times out of 100, they are shameful waste, and more than waste, for they entail a great cost for their removal. The town of Leeds pays about 14,000*l*. a year for the scavenging of the streets and the emptying of ashpits. Nearly every house in Leeds supplies in the way of cinders at least twice as much ashpit refuse as it might do, were the fireplaces properly constructed. The ashpit refuse of Leeds is burned in a "Destructor," and the cinders in the refuse provide, not only heat enough for its reduction to a mineral residue, but spare heat for driving two 60-horse power engines, and for consuming a reasonable amount of pigs, &c., killed by or on account of disease.

These three great evils, evils affecting not only individuals, but the community, waste of fuel and heat, production of soot, production of cinders, are a direct result of the violation of the correct principles in fireplace construction.

Let us next enquire what are the principles which promote good combustion in an open fireplace—i. e. what are the conditions which are essential to enable fuel to give out to a room "good money's worth in heat." That such a result may be obtained, fuel must burn *well* but *not rapidly*. Two things in combination are essential to the combustion of fuel—a supply of oxygen, and a high temperature—i. e. plenty of heat around the fuel. If fuel be burned with a hot jacket around it, a very moderate amount of oxygen will sustain combustion, and if the supply of oxygen be moderate, combustion is slow. Burn coal with a chilling jacket around it, a rapid conductor like iron, and it needs a fierce draught of oxygen to sustain combustion, and this means rapid escape of actual heat, and also of potential heat in unburnt gases and smoke, up the chimney. This is the key to the whole position; this is the touchstone by which to test the principles of fireplace construction.

Few people probably realise the exact conditions of combustion, which may be well illustrated from the process of manufacture of coal gas. In coal we have three kinds of constituents. One mineral, incombustible, seen in the ash residue, which for good coal amounts to barely 3 per cent. The second, volatile, and which, under the influence of heat becoming gaseous, appears in an open fire as tall flame and smoke, and, where combustion is imperfect, produces soot. The third constituent is carbon or charcoal, familiarly known as coke or cinder, and when burning gives a short shallow blueish flame. The carbon and the volatile portions can be raised to a high temperature, and still will not burn unless oxygen be brought into contact with them.

In the manufacture of gas, coal is raised to a high temperature, and the gases are driven off by roasting the coal in an oven from which air, i. e. oxygen, is shut out. The gases are conducted away, cooled, purified, and stored for future use in a gasometer; the combined carbon and mineral residue, being non-volatile, is cooled down before being exposed to the air, and is sold as coke. Here we have a striking proof of the fact that high temperature in fuel does not of itself involve combustion. If air were admitted to the red-hot coke, or to the gases as they escape in their heated condition from the furnace, they would burn. But when coke has become cold, and the gases are cold, as in a gasometer, no amount of oxygen will of itself start combustion.

The deduction from all this is, that complete oxydation, i. e. good combustion, is possible only when the fuel and gases are at a high temperature, and that high temperature of fuel does not produce combustion until oxygen is introduced,—therefore we can have a high temperature of fuel, without rapid combustion, provided we control and limit the supply of oxygen. If we have thoroughly grasped these elementary facts, we shall be in a position to understand the points to be aimed at in the construction of a fireplace.

My attention was first directed to the question of waste of fuel at the time of the coal famine some twelve years ago. I read in the 'Times,' and acted upon the suggestion, made, I believe, by the late Mr. Mechi, to economise coal by inserting an iron plate on the grid under the fuel so as to cut off all draught through the fire. This undoubtedly induced slow combustion, and economised fuel, but the fire was dull, cold, and ineffective. The plan was abandoned. It taught me, however, the fact that combustion could be controlled by cutting off the underdraught, but I did not then see why combustion was spoiled. The reason was that the under surface of the fire was chilled, and the fuel lost its incandescence owing to the rapid loss of heat through the iron towards the open hearth chamber. To some persons even now "Slow combustion stoves" are an abomination, and are supposed to be synonymous with bad combustion.

The next stage in my fireplace education was the adoption of the Abbotsford grate. I thereby learnt that the reason why an Abbotsford grate was an advance upon the iron plate lay in the fact that the solid firebrick bottom stored up heat and enabled the fuel to burn more brightly resting upon a hot surface—not upon a cooling iron plate. But Abbotsford grates, and the other class of grates with solid firebrick bottoms, the Parson's grates, have disadvantages. They are apt to become dull and untidy towards the end of the day, and do not burn satisfactorily with inferior coal. There is a better thing than a solid firebrick bottom, and that is the chamber under the fire closed in front by an "Economiser."

The history of the next, the most important stage of my fireplace education, was as follows:—

Some five years ago I made, somewhat accidentally, the discovery that the burning of coal in an ordinary fireplace could be controlled and retarded by the adoption of a very simple and inexpensive contrivance, applicable to nearly every existing grate, and that this result could be attained without impairment of, and often with increase of, the heating power of the fire. This contrivance, which I have named an "Economiser," was simply a shield of iron, standing on the hearth, and rising as high as the level of the grid at the bottom of the grate, converting the hearth space under the fire into a chamber closed by a movable door.

The effect was twofold. The stream of air, which usually rushes through the bottom of the fire, and causes for a short time rapid combustion at a white heat, was thereby cut off, and the air *under the fire* was kept stagnant, the heated coal being dependent for its combustion on the air passing *over the front and the upper surface.* The second point was that this boxing up rendered the chamber hotter, and this increased temperature beneath the fire-grate, i. e. under the fuel, added so materially to the temperature of the whole, even of the cinders coming into contact with the iron grid, that the very moderate supply of oxygen reaching the front and upper surface of the fuel was sufficient to maintain every portion in a state of incandescence. Moreover, I observed that combustion was going on at an *orange*, not at a *white*, heat.

Let us contrast a white with the orange heat. A *white heat* in a fire means *rapid combustion, owing to the strong current of air, oxygen, which passes under the grate, through the centre of the fire*, and up the chimney. As soon as the heart of the fire has been rapidly burnt away at a white heat, the fuel cools; the iron grid cools also; and the cinders in contact with the grid are chilled below combustion point. They then cease to burn, and the bottom of the fire becomes dead and choked. The poker must now be brought into play to clear away the dead cinders, and to re-open the slits in the choked grid. New coal is added to the feeble remnant of burning embers, with no reserve of heat in the iron surroundings; and in time, and perhaps very slowly, the fire revives, and rapid combustion sets in afresh under the influence of the renewed current of oxygen passing through the heart of the fire. An *orange heat* means that the coke, i. e. the red-hot cinder, is burning with a *slowly applied stream of oxygen, a degree of combustion which is only possible when the coal is kept warm by the hot chamber beneath*, and by a reasonable limitation of loss of heat at the back and sides by firebrick, either in contact with the fuel, or at least close behind the iron surrounding it. This effect is seen, partially, in the grates with solid firebrick bottom, but far more perfectly in the grates with the chamber closed by the "Economiser."

This hot chamber has the following effects:—The incandescent coal remains red-hot from end to end of the grate, until nearly all is consumed, thus maintaining a larger body of the fuel in a state to radiate effective heat into a room. The cinders on coming into contact with the iron grid also remain red-hot, and so continue to burn away until they fall through the grid as a fine powder. This allows the fire to burn clearly all day long almost without poking. When the fire is low, and new coal is added, the reserve of heat in the hot chamber is such that the addition of cold fresh fuel does not temporarily quench the embers, and the fire is very quickly in a blaze after being mended.

Having made the discovery by the observation of a grate supplied to me with an "Economiser," the value of which, I suspect, was hardly appreciated by the makers, I applied "Economisers" one by one to all my grates, kitchen included. The result surpassed my expectations. There was a saving of at least a fourth of my coal. The experience of many friends, who at my advice adopted the system, confirmed my own results. It was therefore clear to me that

I was bound to make widely known a discovery which was fraught with such benefit to myself, and was likely to prove a great boon to the public.

My chief aim hitherto has been to persuade the public to apply the "Economiser" to existing fireplaces. After steady exertions for four years, some impression has been made on the inertia of the public, and extensive trials of the "Economiser" are taking place in many parts of the country. To-day, however, my aims are more complete. It is my wish to advocate not one principle alone, although that is the cardinal one, but to urge all the best principles which enter into the construction of a really effective fireplace, and to induce those whom it may concern to replace bad by an entirely new construction, right in every point.

The rules of construction which I shall lay down have been arrived at entirely by my own observation of what appeared to be the best points in various fireplaces. It was, therefore, no less a satisfaction to me than a surprise to discover, on reading Rumford's work in preparation for this lecture, that nothing which I have to advocate is new, but that every principle, and the "Economiser" is hardly an exception, was advocated no less enthusiastically by him at the very commencement of this century.

Having considered the principles that should guide us, we are now prepared to lay down strict rules which should be acted upon in the construction of fireplaces. I trust that what I have said has so far commended itself to your judgment that the thirteen rules here drawn up will command your hearty assent, and in due time will win their way into the confidence of our architects, our builders, and the public.

Rule I. "*As little iron as possible.*"—The only parts of a fireplace that are necessarily made of iron are the grid on which the coal rests, and the bars in front. The "Economiser," though usually made of iron, from convenience in construction, might be of earthenware, and so would be more perfectly in harmony with this rule. On this point Rumford speaks most emphatically:—"Those (grates) whose construction is the most simple, and which, of course, are the cheapest, are beyond comparison the best, *on all accounts*. Nothing being wanted in these chimneys but merely a grate for containing coals, and, additional apparatus being not only useless but very pernicious, all complicated and expensive grates should be laid aside, and such as are simple substituted in their stead. In the choice of a grate beauty and elegance may easily be united with perfect simplicity. Indeed, they are incompatible with everything else." (III. 517.) Again he says, "Iron, and in general metals of all kinds, are to be reckoned amongst the very worst materials that it is possible to employ in the construction of a fireplace."

Rule II. "*The back and sides of the fireplace should be of brick, or firebrick.*"—Brick retains, stores, and accumulates heat, and radiates it back into the room, and keeps the fuel hot. Iron lets heat slip through it up the chimney, gives very little back to the room, and chills the fuel. On this point also Rumford speaks very strongly. "The best materials I have hitherto been able to discover are firebrick and common bricks and mortar." "The fuel, instead of being employed to heat the room *directly* or by the direct rays from the fire, should be so disposed or placed as *to heat the back and sides of the grate*, which must always be constructed of firebrick or firestone, and *never of iron or any other metal*." (III. 345.)

Rule III. "*The firebrick back should lean over the fire, not lean away from it*," as has been the favourite construction throughout the kingdom. The lean-over not only increases the power of absorbing heat from rising flame—otherwise lost up the chimney—but the increased temperature accumulated in the firebrick raises the temperature of gases to combustion point, which would otherwise pass up the chimney unconsumed, and thus be lost. Rumford discovered accidentally the value of this "lean-over," and at once realised its immense importance. He does not, however, seem to have carried out his intention of working out for general adoption this form of back.

He first of all condemns to alteration all firebacks which lean away from the fire. "It frequently happens that the iron backs of grates are not vertical, but inclined backwards. Where the grates are wide, and can be filled up with firebrick, the inclination of the back will be of little consequence, since, by making the firebrick in the form of a wedge, the front may be made perfectly vertical, the iron back being hid in the solid work of the fireplace. If the grate be too shallow to admit of any diminution, it will be best to take away the iron back entirely, and cause the vertical back of the fireplace to serve as the back to the grate." (III. 521.)

He next describes his discovery of the value of the "lean-over":—"In this case I should increase the depth of the fireplace at the hearth to 12 or 13 inches, and should build the back perpendicular to the height of the top of the burning fuel, and then, sloping the back by a gentle inclination forward, bring it to its proper place, that is to say, *perpendicularly under the back part of the throat of the chimney*. This slope (which will bring the back forward 4 or 5 inches, or just as much as the depth of the fireplace is increased), though it ought not to be too abrupt, yet it ought to be quite finished at the height of 8 or 10 inches above the fire, otherwise it may perhaps cause the chimney to smoke.

"Having been obliged to carry backward the fireplace in the manner here described, in order to accommodate it to a chimney whose walls in front were remarkably thin, I was surprised to find, upon lighting the fire, that it appeared to give out more heat into the room than any fireplace I had ever constructed. This effect was quite unexpected; but the cause of it was too obvious not to be immediately discovered. The flame rising from the fire broke against the part of the back which sloped forward over the fire, and this part of the back being soon very much heated, and in consequence of its being very hot (and when the fire burned bright it was frequently quite red-hot), it threw off into the room a great deal of radiant heat. It is not possible that this oblique surface (the slope of the back of the fireplace) could have been heated red-hot *merely* by the radiant heat projected by the burning fuel; for other parts of the fireplace nearer the fire, and better situated for receiving radiant heat, were never found to be so much heated; and hence it appears that the combined heat in the current of smoke and hot vapour which rises from an open fire *may be*, at least *in part*, stopped in its passage up the chimney, changed into radiant heat, and afterwards thrown into the room.

"This opens a new and very interesting field for experiment, and bids fair to lead to important improvements in the construction of fireplaces. . . . . But, as I mean soon to publish a particular account of these fireplaces, with drawings and ample directions for constructing them, I will not enlarge further on the subject in this place. It may, however, not be amiss just to mention here, that these new invented fireplaces not being fixed to the walls of the chimney, but merely set down upon the hearth, may be used in any open chimney; and the chimneys altered or constructed on the principles here recommended are particularly well adapted for receiving them." (III. 526.)

Of recent years "lean-over" backs have been re-invented and sparingly used. The "Milner" back, invented by a Lincolnshire clergyman, and adopted by Barton and Co., is excellent. It burns fuel well, and gives out a great heat. But it is extravagant in consumption unless controlled by the "Economiser."

Captain Douglas Galton saw the virtue of the "lean-over," and adopted it in the grate which goes by his name. The "Bee-hive" back was the same in principle and very good, and having a very small grid, was economical.

The "Rifle" back, adopted by Nelson and Sons, of Leeds, gives an admirable fire, little short of perfection; but observation shows that the "tall" flame extends far beyond the bend, and is therefore soon lost as a heating factor, the heat being wasted in the chimney.

From the commencement of my study of the fireplace question the value of the "lean-over" has not only taken firm hold of my fancy, but my sense of its importance has been growing in intensity, until I saw that the best construction must show the greatest possible extent of "lean-over" that could be obtained without sacrifice of other important details of construction. How to accomplish this will appear in considering the Fifth Rule.

RULE IV. "*The bottom of the fire, or grating, should be deep from before backwards, probably not less than* 9 *inches for a small room, nor more than* 11 *inches for a large room.*"—This is a corollary to Rule III. We cannot possibly have the back of the fireplace overhanging the fire when there is a shallow grid. If for no other reason than the demands of the "lean-over," depth of fire space is essential. But there is gain, thereby, in another direction. It affords plenty of room for the burning fuel to lie down close to the grid, and away from swift air currents, and prevents the tendency of the fire to burn hollow.

On this point Rumford has a word to say:—"But as many of the grates now in common use will be found too large when the fireplaces are altered and improved, it will be necessary to diminish their capacities by filling up with pieces of firebrick. But in diminishing the capacities of grates, care must be taken not to make them *too narrow*, i. e. too shallow.

"The proper depth for grates for rooms of middling size will be from six to eight inches. But where the width (i. e. depth) is not more than five inches, it will be very difficult to prevent the fire going out." (III. 520.)

"Where grates designed for rooms of middling size are longer (and broader) than 14 or 15 inches, it will always be best to diminish their length by filling them up at their two ends by firebrick." (III. 522.)

Rule V. "*The sides or 'covings' of the fireplace should be inclined to one another as the sides of an equilateral triangle*" (Fig. 2, p. 350).—The working out of this rule has cost me much thought and experiment. It was worked out more or less empirically with a view to attain certain objects, and, having attained them, I discovered that I had unwittingly selected the sides of an equilateral triangle. It is of some importance, and may be of interest, to tell how the question arose. In my earlier fireplaces the sides or "covings" were parallel to each other, and had the defect that they radiated most of their heat from one to the other, not into the room, with the probable result that much of such heat would eventually escape up the chimney.

It was clear then that the sides must be set at an angle with the back, so as to face towards the room. But at what angle? My first experiments were determined by the shape of the corner bricks which were in the market. These determined the inclination of the sides to be such that, if prolonged, they would meet at a right angle. This is the angle laid down by Rumford as the angle of selection, but as the largest angle admissible in a good fireplace. This angle, however, brought me into difficulties with my "lean-over" back. The openness of the angle made the back, as it ascended, spread out so rapidly that what was gained in width was lost in height. Moreover, my critics objected to its appearance as ugly. What then should determine the inclination of the sides? The point was thus determined. Seeing that a heated brick throws off the greatest amount of radiant heat at a right angle with its surface, the "covings" should be at such an inclination to each other that the perpendicular line from the inner margin of one "coving" should just miss the outer margin of the opposite "coving." Where the "covings," as in my earlier attempts and in Count Rumford's fireplaces, are at a right angle to each other, this perpendicular line misses the opposite margin by several inches.

It was clear, therefore, that the inclination might be made more acute. Guided by this idea, and having determined the principle on which the shape of the grate should depend, an inclination was arrived at which turned out to be an angle of 60°, i. e. the inclination of the sides of an equilateral triangle.

Count Rumford came very nearly to the same conclusions:—"I have said, in my essay on chimney fireplaces, that where chimneys are well constructed and well situated, and have never been apt to smoke, in altering them the 'covings' may be placed at an angle of 135 degrees with the back; but I have expressly said that they should never exceed that angle, and have stated at large the bad consequences that must follow from making the opening of a fireplace very wide, when its depth is very shallow." (V. p. 510.)

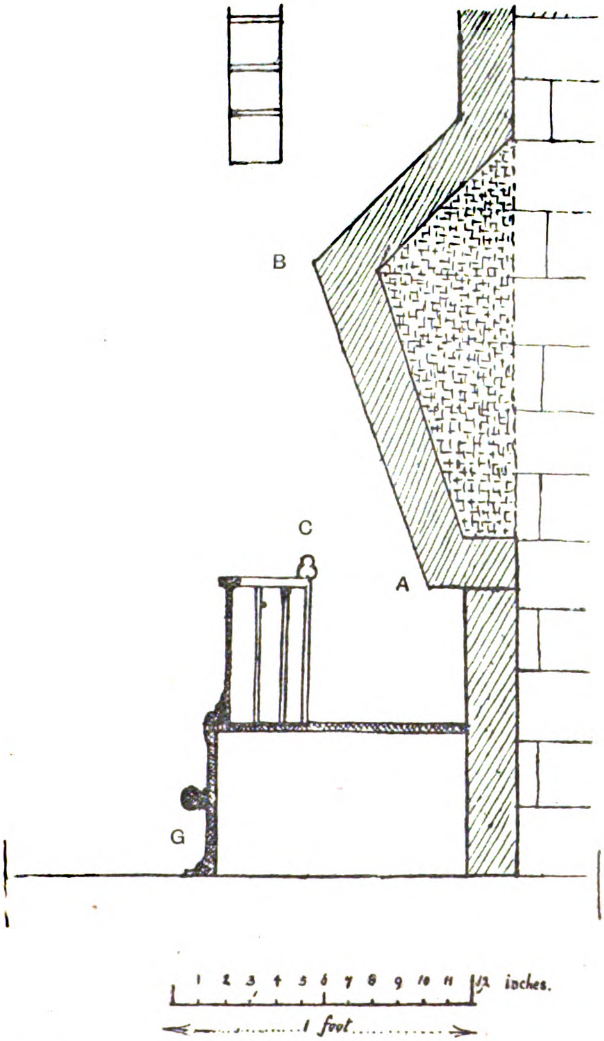
Rule VI. "*The 'lean-over' at the back should be at an angle of* 70°" (Fig. 1).—Commencing at a level (A) corresponding with the top of the front bars, and leaning forward at an angle of 70° with the horizontal line of the hearth, the back should rise to such a point that the angle where it returns towards the chimney (B) should be vertically over the insertion (C) of the cheeks of the fire-grate. This angle (B) will be about 28 inches from the hearth, or 16 inches from the top of the fire, and about 3½ to 4½ inches from the front line of the fireplace, according to the size of the grate. These points will be obvious from the vertical section of the fireplace here shown, and from C, Fig. 2.

Note.—So far, in the fireplaces built after my rules, the height of the grid from the hearth has been taken at two bricks, or 6 inches, and the height of the bars from the grid also at two bricks, or 6 inches. It follows, therefore, that the lean-over commences at 12 inches from the hearth. It is possible that a better angle than 70° may eventually be found—such as an angle of 60°—but commencing a few inches above the fire so as *not to lower the angle* B where the lean-over returns to the chimney.

Rule VII. "*The shape of the grate should be based upon a square described within an equilateral triangle, the size to vary in constant proportion to the side of the square*" (Fig. 2).—The shape of the grate, or grid, is arrived at in the following way:—Describe a square D, of which the sides shall be 8, 9, or 10 inches, according to the size of the room, within an equilateral triangle E, the two sides of which shall represent the "covings" of the fireplace, and the base the front line of the fireplace. From each front angle of the square carry a line from D to C, to the "covings" or sides of the triangle, at an angle of 45° with the front line of the fireplace. These two lines, with the side of the square from which they are drawn, form the front of the grid. The back line of the grid does not correspond with the corresponding side of the square, but is carried 1½ inches further back, so as to give greater depth to the grate, and allow the firebrick back to overhang the back of the grid to the extent of 1½ inches (see A, Fig. 1) before it ascends as the "lean-over."

The diagram of the grate, with the square and triangle on which it is based in dotted lines, will, I hope, make this description sufficiently intelligible.

FIG. 1.

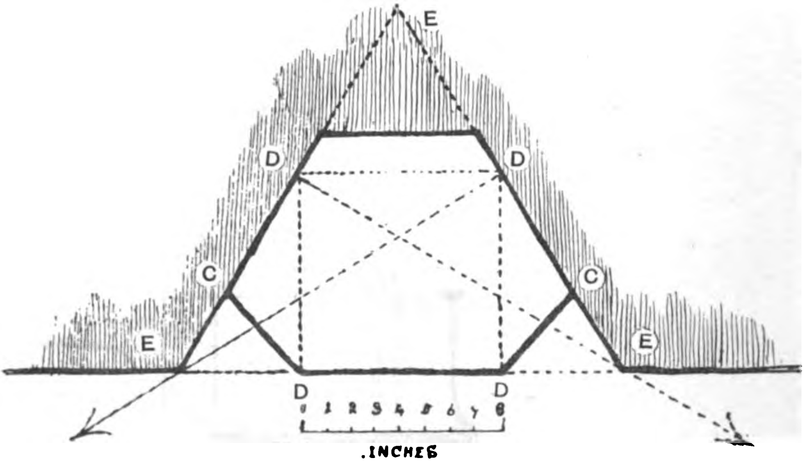


Whenever a grate on this principle proves too hot for a room, and in summer when a smaller fire is needed, the size should be reduced *in width* by triangular firebricks at each side, which reduce the fire space to a square, with the addition of the 1½-inch space under the back. This rule secures sufficient depth from front to back, and a constant proportion between depth and width, whatever be the size of grate.

Rule VIII. "*The slits in the grating, or grid, should be narrow, perhaps ¼ inch for a sitting-room grate and good coal, ⅜ for a kitchen grate and bad coal.*"—When the slits are larger, small cinders fall through and are wasted.

Rule IX. "*The front bars should be* vertical, *that ashes may not lodge and look untidy;* narrow, *perhaps ¼-inch in thickness, so as not to obstruct heat; and* close together, *perhaps ¾-inch apart, so as to prevent coal and cinder from falling on the hearth*" (Fig. 3).—It is too soon to judge as to the lasting powers of ¼-in. bars. Those in one of my own grates are round, and after 4½ months' daily wear, show no sign of burning away.

Fig. 2.

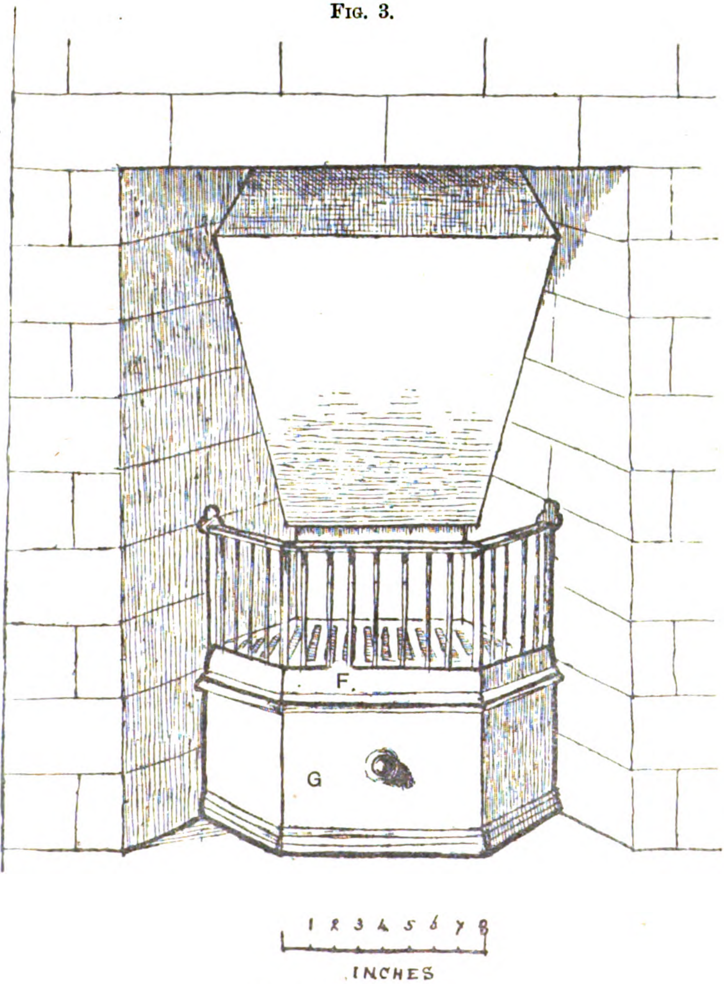


Flat bars, ¼ in. × ½ in., or even by ¾ in., might perhaps resist fire better, if the ¼-in. round bars burn away. The bars are so arranged that if one fails, it can easily be renewed. I have round bars about ⅓ in. in diameter at present on trial in my kitchen range.

Rule X. "*There should be a rim* 1 *inch or* 1½ *inch in depth round the lower insertion of the vertical bars*" (Fig. 3).—The object of this is to conceal the ash at the bottom of the fire, and to enable the front cinders to burn away completely by protecting them from the cold air. This rim (F) contributes greatly to tidiness, and as a rule will prevent the need of any sweeping up of the hearth during the day.

Rule XI. "*The chamber under the fire should be closed by a shield or 'Economiser'*" (G, Figs. 1 and 3).—This has been already spoken of, and described as the central principle which enhances greatly the value of all the rest.

Fig. 3.



Front View of Fireplace.

Rule XII. "*Whenever a fireplace is constructed on these principles, it must be borne in mind that a greater body of heat is* *accumulated about the hearth than in ordinary fireplaces. If there be the least doubt whether wooden beams may possibly run under the hearthstone, then an ashpan should be added, with a double bottom, the space between the two plates being filled with artificial asbestos, 'slagwool,' 2 inches in thickness."*

Rule XIII. "*A fireplace on this construction must not be put up in a party wall, where there is no projecting chimney breast, lest the heated back should endanger woodwork in a room at the other side.*"

Having now worked up Rules for the construction of an effective fireplace, let us consider what benefits result.

1.—*Economy of Fuel.* I have already stated that my own experience of the application of the "Economiser" to all my original fireplaces, including kitchen and scullery, was a saving of more than one-fourth. Friends who have followed my advice report variously from a sixth to one-third. The saving in the Leeds Infirmary, according to returns supplied to me by Mr. Blair, the General Manager, has been nearly a sixth, amounting to nearly 100 tons in the year. What the saving in the fireplaces constructed on the best rules may be I cannot say, probably about the same degree of saving, with a large increase of heat given into the room. My conviction is that such fireplaces make one ton of coal give out as much heat into a room as two tons would yield if burnt in the worst forms of the nearly obsolete register stove. Need more be said about economy of fuel?

2.—*Reduction of Soot.* This is, perhaps, from a national point of view, the most important point in connection with our subject—and yet it is the portion of it in which my evidence is the most defective. I can only offer you my general impression that there is a very important reduction in the amount of soot, an impression based upon observation of the smoke issuing from chimneys where "Economisers" are in use, and of the diminution of soot falling about my own house, which is confirmed by the testimony of Miss Gordon, Lady Superintendent of the Leeds Infirmary, as to the lessened amount of soot which finds its way into the wards.

3.—*Reduction of Ashpit Refuse.* This point is clearly proved by the fine snuff-like powder, free from cinders, which I show; and by the fact that the whole produce in the ashpit of my kitchen fireplace for one week was contained in one ashpan, and weighed 15 lbs. Surely this is a fact for our local authorities to grasp.

Danger of Fire.—Seeing that improved fireplace construction involves increased heat about the hearth, an actual danger of fire will be created where the hearthstone rests on wood, unless the hearth itself be protected. It was therefore my duty to find out a means of protecting the hearth. With this view, experiments have been made with ashpans with double bottoms and a small air-space between the ashpan and the hearth. The results are shown in the specimens of cotton wool, wood, &c., which have been exposed under ashpans of various constructions. My conclusion is that two inches of artificial asbestos at the bottom of an ashpan would render any hearth safe. Such an ashpan may be named a "Hearth Protector." Another caution should be given against erecting one of these improved fireplaces where there is no projecting chimney breast, lest there should be insufficient depth of brick between the back of the fire and the woodwork of a room at the other side.

"*Kitchen Refuse.*"—In some households there are certain portions of kitchen refuse which are apt to find their way into the dust-bin, instead of the pig-tub. You here see the remains of refuse, consisting of celery stalks, potato parings, &c., which have been roasted in a wire cage underneath my kitchen fire in the chamber closed by the "Economiser." The wire cage is necessary to allow the heat to reach the under surface of the refuse.

Having now for four years done my best to persuade the public to take measures in reference to fireplaces which will confer upon them a saving in the cost of fuel, a saving in the labour of servants, an increase in the warmth and comfort of rooms, a lessening of the soot in the atmosphere of towns, and a possibility of reduction of scavenging rates, it is no little satisfaction to feel that my views are at last making way, and acquiring a momentum of their own; and I am encouraged to hope that the time is not far distant when the Committees of Public Institutions, and the Directors of Railway Companies, following the example of the Weekly Board of the Leeds Infirmary, will feel it their duty to weigh their value, and, if they prove true, to adopt them; and perhaps even the Smoke Abatement Society may be induced, as discharging a function for which it exists, to put them to the test of scientific experiment.

It only remains for me now to bring my address to a conclusion with the words of the Roman Shakespeare,—

> "*Non fumum ex fulgore, sed ex fumo dare lucem.*"
> Hor. Ars. Poet.

—which I will translate in the words of one of our greatest Latin scholars, the late Professor Conington,

> "Not smoke from fire my object is to bring,
> But fire from smoke, a very different thing."

[T. P. T.]

WEEKLY EVENING MEETING,

Friday, February 12, 1886.

The Right Hon. Lord Rayleigh, M.A. D.C.L. LL.D. F.R.S. Manager and Vice-President, in the Chair.

Professor Osborne Reynolds, M.A. F.R.S.

Experiments showing Dilatancy, a property of Granular Material, possibly connected with Gravitation.

In commencing this discourse, the author said, My principal object to-night is to show you certain experiments which I have ventured to think would interest you on account of their novelty, and of their paradoxical character. It is not, however, solely or chiefly on account of their being curious that I venture to call your attention to them. Let them have been never so striking, you would not have been troubled with them had it not been that they afford evidence of a fact of real importance in mechanical philosophy.

This newly recognised property of granular masses which I have called dilatancy will, it may be hoped, be rendered intelligible by the experiments, but it was not by these experiments that it was discovered.

This discovery, if I may so call it, was the result of an attempt to conceive the mechanical properties a medium must possess in order that it might fulfil the functions of an all-pervading æther—not only in transmitting waves of light, and refusing to transmit waves like those of sound, but in causing the force of gravitation between distant bodies, and actions of cohesion, elasticity, and friction between adjacent molecules, together with the electric and magnetic properties of matter, and at the same time allowing the free motion of bodies.

It will be well known to those who attend the lectures in this room, that although a vast increase has been achieved in knowledge of the actions called the physical properties of matter, we have as yet no satisfactory explanation as to the *prima causa* of these actions themselves; that to explain the transmission of light and heat it has been found necessary to assume space filled with material possessing the properties of an elastic jelly, the existence of which, though it accounts for the transmission of light, has hitherto seemed inconsistent with the free motion of matter, and failed to afford the slightest reason for the gravitation, cohesion, and other physical properties of matter. To explain these, other forms of æther have been invented, as in the corpuscular theory and the celebrated hypothesis of La Sage, the impossibilities of which hypotheses have been finally proved by the late Professor Maxwell, to whom we owe so much of our definite knowledge of the fundamental physics. Maxwell insisted on the fact, that even if each of the physical properties could be explained by a special æther, it would not advance philosophy, as each of these æthers would require another æther to explain its existence, *ad infinitum*. Maxwell clearly contemplated the existence of one medium, but it was a medium which would cause not one but all the physical properties of matter. His writings are full of definite investigations as to what the mechanical properties of this æther must be to account for the laws of gravitation, electricity, magnetism, and the transmission of light, and he has proved very clear and definite properties, although, as he distinctly states, he was unable to conceive a mechanism which should possess these properties.

As the result of a long-continued effort to conceive a mechanical system possessing the properties assigned by Maxwell, and further, which would account for the cohesion of the molecules of matter, it became apparent that the simplest conceivable medium—a mass of rigid granules in contact with each other—would answer not one but all the known requirements, provided the shape and mutual fit of the grains were such, that while the grains rigidly preserved their shape, the medium should possess the apparently paradoxical or anti-sponge property of swelling in bulk as its shape was altered.

I may here remark, that if æther is atomic or granular, that it should be a mass of grains holding each other in position by contact like the grains in the sack of corn is one of only two possible conceptions; the other being that of La Sage, or the corpuscular theory that the grains are free like bullets moving in space in all directions.

Nor, in spite of its paradoxical sound, is there any great difficulty of conceiving the swelling in bulk. When the grains are in contact, it appears at once that the mechanical properties of the medium must be to some extent affected by the shape and fit of the grains. And having arrived at the conclusion that in order to act the part of æther, this shape and fit must be such that the mass could not change its shape without changing its volume or space occupied, the next thing was to see what possible shape could be given to the grains, so that while these rigidly preserved their shape, the medium might possess this property of dilatancy.

It was obvious that the grains must so interlock, that when any change of shape of the mass occurred, the interstices between the grains should increase. This would be possessed by grains shaped to fit into each other's interstices in one particular arrangement.

In an ordinary mass of brickwork or masonry well bonded without mortar, the blocks fit so as to have no interstices; but if the pile be in any way distorted, interstices appear, which shows that the space occupied by the entire mass has increased (*shown by a model*).

At first it appeared that there must be something special and systematic, as in the brick wall, in the fit of the grain of æther, but subsequent consideration revealed the striking fact, *that a medium* *composed of grains of any possible shape possessed this property of dilatancy so long as one important condition was satisfied.*

This condition is that the medium should be continuous, infinite in extent, or that the grains at the boundary should be so held as to prevent a rearrangement commencing. All that is wanted is a mass of hard smooth grains, each grain being held by the adjacent grains, and the grains on the outside prevented from rearranging.

Smooth hard spheres arranged as an ordinary pile of shot are in their closest order, the interstices occupying a space about one-third that occupied by the spheres themselves. By forcing the outside shot so as to give the pile a different shape, the inside spheres are forced by those on the outside, and the interstices increase. Thus by shaping the outside of the pile, the interstices may be increased to any extent until they occupy about nine-tenths the volume of the spheres: this is the most open formation. A further change of shape in the same direction causes a contraction of the interstices until a minimum volume is reached, and then again an expansion, and so on. The point to be realised is that in any of these arrangements if the whole of the spheres on the outside of the group are fixed, those inside will be fixed also. (*Shown by a model.*)

An interior portion of a mass of smooth hard spheres therefore cannot have its shape changed by the surrounding spheres without altering the room it occupies, and the same is true for any granular mass, whatever be the shape of the grains.

Considering the generality of this conclusion, the non-discovery of this property as existing in tangible matter requires a word of explanation.

The physical properties of elasticity, adhesion, and friction so far render the molecules of ordinary matter incapable of behaving as a system of parts with the sole property of keeping their shape, and so prevents evidence of dilatancy in solids and fluids. This is quite consistent with dilatancy in the æther, for the properties of elasticity, cohesion, and friction in tangible matter are due to the presence of the æther, so that it would be illogical for the elementary atoms of the æther to possess these properties.

This although a sufficient reason why dilatancy has not been recognised as a property of solid and fluid matter, does not explain its non-existence in masses of solid, hard, free grains, as of corn, shot, and sand. To understand why it has not been observed in these, it must be remembered that to ordinary observation these present only an outside appearance, and that the condition essential for dilatancy, that the outside grains should not be free to rearrange, is seldom fulfilled. Also these granular forms of matter, though commonplace, have not been the subjects of physical research, and hence such evidence as they do afford has escaped detection.

Once, however, having recognised dilatancy as a universal property of granular masses, it was obvious that if evidence of it was to be sought from tangible matter, it must be sought in what have hitherto been the most commonplace and least interesting arrangements. That an important geometrical and mechanical property of a material system should have been hidden for thousands of years, even in sand and corn, is such a striking thought that it required no little faith in mechanical principles to undertake the search for it, and although finding nothing but what was strictly in accordance with the conclusions previously arrived at, the evidence obtained of this long-hidden property was as much a matter of visual surprise to the lecturer as it can be to any of the audience.

To render the dilatancy of a granular mass evident, it was necessary to accomplish two things: (1) the outside grains must be controlled so that they could not rearrange, and this without preventing change of shape and bulk of the mass; (2) the changes of bulk or volume of the mass, or of the interstices between the grains, must be rendered evident by some method of measurement which did not depend on the shape of the mass.

A very simple means—*a thin indiarubber* envelope or boundary—answered both these purposes to perfection. The thin indiarubber closed over the outside grains sufficiently to prevent their change of position, and the impervious character of the bag allowed of a continuous measure of the volume of the contents, by measuring the quantity of air or water necessary to fill the interstices.

Taking an indiarubber bag which will hold six pints of water, without stretching, and having only a small tubular aperture, getting it quite dry, and putting into it six pints of dry sea sand, such as will run in an hour-glass, sharp river sand, dry corn, shot, or glass marbles, it presents no very striking appearance, but all the same when filled with any of these materials, it cannot have its form changed as by squeezing between two boards without changing its volume. These changes of volume are not sufficient to be noticeable while the squeezing is going on, but they may be rendered apparent. It is sufficient to do this with the bag full of clean dry Calais sand, such as is used in an hour-glass.

The tube from the bag is connected with a mercurial pressure-gauge, so that the bag is closed by the mercury.

The actual volume occupied by the quartz grains is four and a half pints. The remaining space, one and a half pints, is occupied by the interstices between the grains in their closest order; these interstices are full of air, so that three-quarters of the bag are occupied by quartz, and one-quarter by air. Since the bag is closed and no more air can get in if interstices are increased from one pint and a half to two pints, the air must expand, and its pressure will fall from that of the atmosphere to three-quarters of an atmosphere. As soon as squeezing begins, the mercury rises on the side connected with the bag, and steadily rises as the bag flattens until it has risen seven inches, showing that the bag has increased in capacity by half a pint or one-twelfth of its initial capacity.

That by squeezing a porous mass like sand we should diminish the pressure of the air in the pores is paradoxical, and shows the anti-sponginess of the granular material; had there been a sponge in the bag, the pressure of the air would have increased with the squeezing.

This experiment has been mainly introduced to prevent a possible impression that the fluid filling the interstices has anything to do with the dilation besides measuring it.

Water affords a more definite measure of volume than air.

Taking a small indiarubber bottle with a glass neck full of shot and water, so that the water stands well into the neck. If instead of shot the bag were full of water or had anything of the nature of a sponge in it, when the bag was squeezed the water would be forced up the neck. With the shot the opposite result is obtained; as I squeeze the bag, the water decidedly shrinks in the neck.

This experiment, which you see is on a very small scale, was not designed to show to an audience; it was the original experiment which was made for my own satisfaction, when the idea of dilatancy first presented itself. The result but for the knowledge of dilatancy would appear paradoxical, not to say magical. When we squeeze a sponge between two planes, water is squeezed out; when we squeeze sand, shot, or granular material, water is drawn in.

Taking a larger apparatus, a bag which holds six pints of sand, the interstices of which are full of water without any air—the glass neck being graduated so as to measure the water drawn in. On squeezing the bag with a large pair of pincers, a pint of water is drawn from the neck into the bag. This is the maximum dilation; the grains of sand are now in the most open order into which they can be brought by this squeezing; further squeezing causes them to take closer order, the interstices diminish, and the water runs out into the vessel, and for still further squeezing is drawn back again, showing that as the change of form continues, the medium passes through maximum and minimum dilations.

This experiment may be repeated with granules of any size or shape, provided only they are hard, and shows the universality of dilatancy.

Although not more definite, perhaps more striking evidence of dilatancy is afforded by the means which the non-expansibility of water affords of limiting the volume of the bag. An impervious bag full of sand and water without air cannot have its contents enlarged without creating a vacuum inside it—the interstices of the sand are therefore strictly limited to the volume of the water inside it, unless forces are brought to bear sufficient to overcome the pressure of the atmosphere and create a vacuum. Since then, owing to this property of dilatancy, the shape of a granular mass at its greatest density cannot change without enlarging the interstices, preventing this enlargement by closing the bag we prevent change of shape.

Taking the same bag, the sand being at its closest order—closing the neck so that it cannot draw more water. A severe pinch is put on the bag, but it does not change its shape at all; the shape cannot alter without enlarging the interstices, these cannot enlarge without drawing more water, and this is prevented. To show that there is an effort to enlarge going on, it is only necessary to open a communication with a pressure-gauge, as in the experiment with air. The mercury rises on the side of the bag, showing when the pinch is hardest (about 200 lbs. on the planes) that the pressure in the bag is less by 27 inches of mercury than the pressure of the atmosphere; a little more squeezing and there is a vacuum in the bag. Without a knowledge of the property of dilatancy such a method of producing a vacuum would sound somewhat paradoxical. Opening the neck to allow the entrance of water, the bag at once yields to a slight pressure, changing shape, but this change at once stops when the supply is cut off, preventing further dilation.

In these experiments neither the thickness of the bag nor the character of the fluid has anything to do with the dilation of the contents considered as forming an interior group of a continuous medium, the bag merely controlling the outside members as they would be controlled by surrounding grains, and the fluid merely measuring or limiting the volume of the interstices.

It has, however, been absence of such control of the outside grains and such means of measuring the volume of the interstices that has prevented the dilatancy revealing itself as a general mechanical property of granular material, as a *mechanical* property, because dilatancy has long been known to those who buy and sell corn. It is seldom left for the philosopher to discover anything which has a direct influence on pecuniary interests; and when corn was bought and sold by *measure* it was in the interest of the vendor to make the interstices as large as possible, and of the vendee to make them as small; of the vendor to make the corn lie as lightly as possible, and of the vendee to get it as dense as possible. These interests are obvious; but the methods of getting corn dense and light are *paradoxical* when compared with the methods for other material. If we want to get any elastic material light we shake it up, as a pillow or a feather bed, or a basket of dried fruit; to get these dense we squeeze them into the measure. With corn it is the reverse; it is no good squeezing it to get it dense; if we try to press it into the measure we make it light—to get it dense we must shake it—which owing to the surface of the measure being free, causes a rearrangement in which the grains take the closest order.

At the present day the measure for corn has been replaced by the scales, but years ago corn was bought and sold by measure only, and measuring was then an art which is still preserved. It is understood that the corn is to be measured light, and the method employed is now seen to have made use of the property of dilatancy. The measure is filled over full and the top struck with a round pin called the strake or strickle. The universal art is to put the strake end on into the measure before commencing to fill it. Then when heaped full, to pull the strake gently out and strike the top; if now the measure be shaken it will be seen that it is only nine-tenths full.

Sand presents many striking phenomena well known but not hitherto explained, which are now seen to be simply evidence of dilatancy.

Every one who walks on the strand must have been painfully struck with the difference in the firmness and softness of the sand at different times; letting alone when it is quite dry and loose. At one time it will be so firm and hard that you may walk with high heels without leaving a footprint; while at others, although the sand is not dry, one sinks in so as to make walking painful. Had you noticed you would have found that the sand is firm as the tide falls, and becomes soft again after it has been left dry for some hours. The reason for this difference is exactly the same as that of the closed bags with water and air in the interstices of the sand. The tide leaves the sand, though apparently dry on the surface, with all its interstices perfectly full of water which is kept up to the surface of the sand by capillary attraction; at the same time the water is percolating through the sand from the sands above where the capillary action is not sufficient to hold the water. When the foot falls on this water-saturated sand, it tends to change its shape, but it cannot do this without enlarging the interstices—without drawing in more water. This is a work of time, so that the foot is gone again before the sand has yielded. If you stand still, you will find that your feet sink more or less, and that when you move, the sand becomes wet all round the space you stood on, which is the excess of water you have drawn in, set free by the sand regaining its densest form.

One phenomenon attending walking on firm sand is very striking; as the foot falls, the sand all round appears to shoot white or dry momentarily, soon becoming dark again. This is the suction into the enlarging interstices below the foot, which for the moment depresses the capillary surface of the water below that of the sand.

After the tide has left the sand for a sufficient time, the greater part of the water has run out of the interstices, leaving them full of air, which by expanding allows the interstices to enlarge, and the foot to sink in far enough to make walking unpleasant.

If we walk on sand under water, it is always more or less soft, for the interstices can enlarge, drawing in water from above.

The firmness of the sand is thus seen to be due to the interstices being full of water, and to the capillary action or surface tension of the water at the surface of the sand. This capillary action will hold the water up in the sand for some inches or feet, according to the fineness of the sand. This is shown by a somewhat striking experiment. If sand running in a stream from a small hole in the bottom of a vessel, as in an hour-glass, fall into a vessel containing a slight depth of water, the sand at first forms an island, which rises above the water. The sand which then falls on the top of this island is dry as it falls, but capillary action draws up the water which fills the interstices and gives the sand coherence. The island grows vertically, very fast, and assumes the form of a column, sometimes with branches like a tree or a fern, some inches or even a foot high. The strength of these consists in the surface tension of the water preventing air from being drawn in to enlarge the interstices, which therefore cannot change shape; it is therefore another evidence of dilatancy.

By substituting an impervious envelope for the surface of water, firmness of sand saturated with water may be rendered very striking.

Thin indiarubber balloons, which may be easily expanded with the mouth, afford an almost transparent envelope.

Taking one with about six pints of sand and water closed without air, there being more water than will fill the interstices at the densest, but not enough to allow them to enlarge to the full extent. When standing on the table, the elasticity of the envelope given is a rounded shape. The sand has settled down to the bottom, and the excess of water appears above the sand, the surface of which is free. The bag may be squeezed and its shape altered, apparently as though it had no firmness, but this is only so long as the surface is free. But taking it between two vertical plates and squeezing, at first it submits, apparently without resistance, when all at once it comes to a dead stop. Turning it on to its side, a 56-lb. weight produces no further alteration of shape; but on removing the weight, the bag at once returns to its almost rounded shape.

Putting the bag now between two vertical plates, and slightly shaking while squeezing, so as to keep the sand at its densest, while it still has a free surface, it can be pressed out until it is a broad flat plate. It is still soft as long as it is squeezed, but the moment the pressure is removed, the elasticity of the bag tends to draw it back to its rounded form, changing its shape, enlarging the interstices, and absorbing the excess of water; this is soon gone, and the bag remains a flat cake with peculiar properties. To pressures on its sides it at once yields, such pressures having nothing to overcome but the elasticity of the bag, for change of shape in that direction causes the sand to contract. To radial pressures on its rim, however, it is perfectly rigid, as such pressures tend further to dilate the sand; when placed on its edge, it bears one cwt. without flinching.

If, however, while supporting the weight it is pressed sufficiently on the sides, all strength vanishes, and it is again a rounded bag of loose sand and water.

By shaking the bag into a mould, it can be made to take any shape; then, by drawing off the excess of water and closing the bag, the sand becomes perfectly rigid, and will not change its shape without the envelope be torn; no amount of shaking will effect a change. In this way bricks can be made of sand or fine shot full of water and the thinnest indiarubber envelope, which will stand as much pressure as ordinary bricks without change of shape; also permanent casts of figures may be taken.

I have now shown as fully as time will allow, the experiments which afford evidence of the existence of the property of dilatancy, and how it explains natural phenomena hitherto but little noticed.

Beyond affording evidence of the existence of the property of dilatancy, these experiments have no direct connection with gravitation or the physical properties of matter.

These properties cannot be deduced by direct experiment on granular material, for the simple reason that the grains of the medium which constitutes the æther must be free from friction, while the grains with which we work are subject to friction. These properties can only be deduced by mathematical reasoning, into which I will not drag you to-night. I will merely show you one or two or three facts which may serve to convey an idea of how dilatancy should have such a bearing on the foundation of the universe.

If you look at this diagram, you see it represents a ball surrounded by a continuous mass of grain, the density of the grains being indicated by the depth of colour. If that ball were to grow in volume, it would have to push out the medium on all sides, and in that way it would distort the groups of grains or change their form, causing the interstices to increase; those nearer the ball would be distorted more than those further away. Then the interstices of these would grow the most rapidly, and those adjacent to the ball would first come to their openest order for further growth; these would contract somewhat, those a little further away would reach the openest order, and if the process of growth steadily continued, we should have a series of undulations of density commencing at the ball and moving outwards; the first of these waves of open order would not, however, get beyond half the diameter of the ball away. The diagram represents the interstices that would result if a single grain of the material had grown to the size of the ball, pushing the medium out before it. It is not necessary that the ball should have grown, to produce this result; however the ball were originally placed, if it were moved away from its original place it would assume this arrangement, and with this arrangement it would be free to move. Now, although I cannot attempt to enter upon the relation between the density of the medium and the force of attraction between two bodies in it, I may call your attention to this fact, that the dilation as calculated varies exactly as the force of gravitation, inversely as the square of the distance from an infinite distance till close to the ball, and then goes through several undulations, corresponding exactly to the variations in the attraction of bodies necessary to explain the elasticity and cohesion of molecules. As is shown in the other diagrams, these undulations in density, which may be experimentally produced, not only appear to afford a clear explanation of cohesion, but are the only suggestion of an explanation ever made. And further, similar undulations have been found necessary to explain one of the phenomena of light. My reason for calling your attention to them was partly an experiment, which, although not the most striking, is the most advanced experiment in the direction of dilatancy.

The apparatus is that represented in the diagram; the medium is contained in the large elastic bag; in the middle of this bag is a small hollow elastic ball, which can be expanded by water forced in through a tube passing through the medium and outside ball; the quantity of water which passes in is measured by a mercury gauge, the water being forced in by the pressure of the mercury. The medium between the two balls is sand and water, and is connected with a gauge, the water drawn from which measures the dilation.

The full pressure of 30 inches is on the interior ball, but produces no expansion, because the medium outside cannot dilate as the supply of water is now cut off; opening the tap to admit water to the outer ball, it at once draws water. It has now drawn 3 oz.; in the meantime the mercury has fallen, showing that an ounce and a half was admitted to the interior ball, the expansion of which drew the water into the outer envelope. This experiment is not striking, but it is definite, and enables us to measure the dilation consequent on a given distortion.

It is impossible for me to go further into this explanation, so I will merely state that the ability of the grains of a medium to slide over a smooth surface has been experimentally shown to produce phenomena closely resembling the conduction of electricity, to complete which it is only necessary to construct the medium of two different sorts of grains, different in size or different in shape, the separation of which would afford the two electricities and be a simple way out of the difficulty hitherto found in explaining the non-exhaustibility of the electricity in a body. Hitherto the two electric fluids have been supposed to reside together in the matter of the machine, which, however much has been withdrawn, has never shown signs of exhaustion. In the dilatant hypothesis these electricities are the two constituents of the æther which the machine separates, and it is worth noticing that the ordinary electrical machine resembles in all essential particulars the machines used by seedsmen for separating two kinds of seed, trefoil and rye-grass, which grow together: as long as there is a supply of the mixture, the machine is never exhausted.

This dilatant hypothesis of æther is very promising, although it cannot be put forward as proved until it has been worked out in detail, which will take long. In the meantime it is put forward mainly to excite interest in the property of dilatancy to the discovery of which it has led. This property, now that it has once been recognised, is quite independent of any hypothesis, and offers a new field for philosophical and mathematical research quite independent of the æther.

[O. R.]

WEEKLY EVENING MEETING,

Friday, February 19, 1886.

Sir William Bowman, Bart. LL.D. F.R.S. Manager and Vice-President, in the Chair.

Professor W. H. Flower, LL.D. F.R.S. Director of the British Natural History Museum.

The Wings of Birds.

The power of flying through the air is one of the principal characteristics of the class of Birds. Although some members of the other great divisions of the Vertebrates—the bats among Mammals, the extinct pterodactyle among Reptiles, the flying fishes among Pisces—possess this power in a greater or less degree, these are all exceptional forms, whereas in birds the faculty of flight is the rule, its absence the exception. Among Invertebrates this power is possessed in a very complete degree by the greater number of insects.

In the normal structure of the vertebrate animals there are two pairs of limbs, anterior and posterior, never more. It often happens, however, that one pair, and sometimes both, are suppressed, being rudimentary, functionless, or entirely absent. Flight is always performed by the anterior or pectoral pair, more or less modified for the purpose. The super-addition of wings to arms, as in the pictorial representations of angels, has no counterpart in nature. The wings of the bird, the bat, the pterodactyle, and flying fish, are the homologues of the arms of man, the fore-legs of beasts. In the flying fish the power is gained simply by an enlargement of the pectoral fin, and the function is very imperfect; in the pterodactyle, by immense elongation of one (the outer) finger, and extension of the skin between it and the side of the body; in the bats, by elongation of the four outer fingers, and extension of a web of skin between them and the body. In the bird the flying organ is constructed mainly of epidermic structures, peculiar outgrowths from the surface, called *feathers*—modifications of the same tissue which constitutes the hair, horns, scales, or nails of other animals. Feathers are met with only in birds, and are found in all the existing members of the class, constituting the general covering of the surface of the body.

The framework to which the broad expanse formed by the feathers is attached is composed of bones, essentially resembling those of the fore-limb of other vertebrates. The distal segment, manus, or hand, in the vast majority of birds, has three metacarpal bones and digits, the former being more or less united together in the adult state. The digits appear to correspond with the pollex, index, and medius of the typical pentadactyle manus; the second is always the longest. Both it and the pollex frequently bear small horny claws at their extremity, concealed among the feathers and functionless, but very significant in relation to the probable original condition of the avian wing. These claws are altogether distinct from the large, and often functional, spurs developed in many species from the edge of the metacarpal bones, resembling both in use and situation the corresponding weapons in the hind feet. The third digit does not bear a second phalanx or claw in any existing bird.

The quills, remiges, or flight feathers attached to the bones of the manus (called "primaries"), never exceed twelve in number, and are (as has been recently shown by Mr. Wray) in the very great majority of birds distributed as follows:—Six, or in some few cases (flamingo, storks, grebes, &c.), seven to the metacarpus; of the remainder or digital feathers, one (*ad-digital*) is attached close to the metacarpo-phalangeal articulation, and rests on the phalanx of the third digit; two (*mid-digital*) have their bases attached to the broad dorsal surface of the basal phalanx of the second digit, which is grooved to receive them; the remainder (*præ-digital*) are attached to the second phalanx of the same digit. These last vary greatly in development, in fact their variations constitute the most important structural differences of the wing. In most birds there are two; the proximal one well developed, the distal always rudimentary; but the former may show every degree of shortening, until it becomes quite rudimentary, or even altogether absent, as in *Fringillidæ* and other "nine-primaried" birds, in which there are six metacarpal remiges, one ad-digital, two mid-digital, and no prædigitals, or only a very rudimentary one. The smaller feathers at the base of the quills, called upper and under coverts, have an equally regular arrangement. The webs or vanes of all the flight-feathers are made up of a series of parallel "barbs" which cohere together by means of minute hooklets, and so present a continuous, solid, resisting surface to the air.

Such is the characteristic structure of the wing in almost all carinate birds, whether powerfully developed for flight, as in the eagles, albatrosses, or swifts, or whether reduced in size and power to practically useless organs, as in the extinct great auk, the dodo and its kindred, weka rail, notornis, cnemiornis, &c., most of which, being inhabitants of islands containing no destructive land mammals, appear to have lost the principal inducement, and with it the power, to fly.

In the penguins (*Sphenïscomorphæ*) the feathery covering of the wing entirely departs from the normal type. Each feather is like a flattened scale frayed out at the edges, the barbs are non-coherent and have no hooklets. They form an imbricated covering of both surfaces of the wing, including the broad patagium which extends from the cubital side of the limb, but appear to have no definite relation to the bones, and cannot be divided into distinct groups, corresponding to those described above. The structure of the wing separates the penguins sharply from all the other carinate birds.

The Ratitæ, or birds without keel to the sternum, form another very distinct group, distinguished by the rudimentary or imperfect condition of the remiges or quills, which never have coherent barbs and are therefore unfitted to the purpose of flight. In the ostrich and rhea the bones, though comparatively small, are distinct and complete, and the feathers large and definitely arranged. The emu, cassowary, and apteryx show various degrees of degeneration, which apparently culminated in the dinornis, no trace of a wing-bone of which bird has ever been found. The question which naturally presents itself with regard to these birds is, whether they represent a stage through which all have passed before acquiring perfect wings, or whether they are descendants of birds which had once such wings, but which have become degraded by want of use. In the absence of palæontological evidence it is difficult to decide this point. The complete structure of the bony framework of the ostrich's wing, with its two distinct claws, rather points to its direct descent from the reptilian hand, without ever having passed through the stage of a flying organ. The function of locomotion being entirely performed by powerfully developed hind-legs, and the beak mounted on the long flexible neck being sufficient for the offices commonly performed by hands, the fore-limbs appear to have degenerated or disappeared, just as the hind-limbs of the whales disappeared when their locomotory functions were transferred to the tail. This view is strengthened by the great light that has been thrown on the origin of the wings of the flying birds by the fortunate discovery of the *Archæopteryx* of the Solenhofen beds of Jurassic age, as in this most remarkable animal, half lizard and half bird, the process of modification from hand to perfect flying bird is clearly demonstrated. The three digits which in the existing forms are more or less pressed together and imperfect, still retain their freedom and complete number of phalanges, and are each armed with terminal claws, while the flight feathers and remiges of the cubital, metacarpal, and digital series are fully developed and evidently functional. The earlier stages in which the outer digits were still present, and the feathers imperfectly formed or merely altered scales, are not yet in evidence.

Some conception of the process by which a wing may have been formed may also be derived from the study of the growth of feathers on the feet of some domestic varieties of pigeons and poultry, illustrations of which were shown at the lecture.

[W. H. F.]

WEEKLY EVENING MEETING,

Friday, February 26, 1886.

William Huggins, Esq. D.C.L. LL.D. F.R.S. Vice-President,
in the Chair.

A. A. Common, Esq. F.R.S. *M.R.I.*

Photography as an Aid to Astronomy.

In many kinds of astronomical work the old method of direct observation seems likely to be superseded by the use of photography; and the astronomer of the near future, instead of examining with eye and telescope the various objects of the heavens, will prefer to deal with the automatic records they leave on the sensitive plate. In some work this state of things already exists, and its extension to all kinds seems but a matter of time. It appears strange that any indirect way of seeing an object can be better than the direct way, but in some cases we shall certainly find it to be so. Between the construction of the eye and of the apparatus of the photographer there are many points of great similarity. Both have optical means of producing an image, a camera, or dark chamber to keep out other light than that going to form this image; and both have a similar screen on which this image is received. It is when we come to consider the action of the screen in dealing with the image that the distinctive difference becomes apparent. In the eye the retina receives this image, and in some occult way the impressions are carried to the brain. With the sensitive plate the varying amount of light that makes up the images produces corresponding changes in the chemical nature of the film which allows the reproduction of this image in a visible form afterwards.

As I understand the action of the eye, the retina acts only as a transmitter of the sensations produced on its delicate structure by the image; and the brain records these sensations in a more or less perfect manner according to its capacity to deal with all those sensations that are transmitted; hence the power of the eye is limited by the power of the brain to record, and this is evidently in many cases less than that of the eye to perceive, whilst the power of the eye itself is limited in more than one important point as regards our subject, the retina becoming insensible to an image however perfect if insufficiently illuminated. With the sensitive plate the measure of the effective light of an image is not, as in the eye, the amount going to form the image, but the total amount that can be accumulated by a sufficiently long exposure. Hence, we have the remarkable fact that I have elsewhere before mentioned, that a certain object such as a star or nebula, that would be just beyond the power of the eye, however long the gazing was continued, could be photographed with a sufficiently long exposure; and this holds good whatever optical power be employed to increase the amount of light brought to the eye; as with the same optical power, the power of the sensitive plate, if allowed sufficient exposure, will always be greater. There is from this another consequence:—The enlargement of an image produced by a telescope is limited in one direction by the light, faint objects becoming too faint to be seen if greatly enlarged, the sensitive plate, however, may still utilise this image. There are other points of difference between the action of the retina and of the sensitive plate. The power of the latter is greater to record rays of light of quicker vibration, and it may be possible to obtain photographs of celestial objects radiating light that the eye is not adapted to receive; and it is also quite possible that plates may be prepared that will be sensitive to the visual rays so that the magnitudes of stars in stellar photographs would agree with magnitudes as obtained by the eye.

Though the image seems to be clearly formed in the eye over a large angular extent, the central parts only are clearly seen, and the image has to be traversed across this central part piece by piece to be properly examined.

With the sensitive plate, the image, no matter how complex, acts equally over its extent and records itself with fidelity.

As was well said many years ago by Dr. de la Rue, who has been rightly called the father of astronomical photography, "the sensitive film is a retina that never forgets."

I will try and show to-night how important the difference between the ordinary eye observations and the work done by photography may become; not only in cases where the ordinary visual observations have been used, but in cases where the use of the micrometer attached to the telescope has been the only means of accurate observations. Looking for a moment at the history of our subject, it appears that the earliest application of photography to a celestial object was made by Professor J. W. Draper in 1840 within one year of the announcement by Daguerre of the details of the photographic process with which his name will always be associated. Professor Draper obtained a picture of the moon in twenty minutes, using a lens and a heliostat.

An extract from the minutes of the New York Lyceum of Natural History was to this effect:—

"March 23, 1840.—Dr. Draper announced that he had succeeded in getting a representation of the moon's surface by the daguerreotype. The time occupied was twenty minutes, and the size of the figure about one inch in diameter. Daguerre had attempted the same thing but did not succeed. This is the first time that anything like a distinct representation of the moon's surface has been obtained.—Signed, Robert H. Brownne, Secretary."

Remembering that this entry was made less than one year after the publication of Daguerre's process, the negative statement that Daguerre had failed where Draper had succeeded, is strange; and the allusion to the distinct representation of the moon's surface rather implies that other representations existed. It is, however, the earliest record I can find, and we may consider it the starting-point.

Beyond experimental work little seems to have been done with the daguerreotype.

Some astronomers, notably G. P. Bond in America, assisted by two skilful photographers, with the 15-inch refractor of the Harvard College Observatory, obtained photographs of some of the brighter stars, and also some very fair pictures of the moon, that were exhibited at the 1851 Exhibition in London, and also at a meeting of the R.A.S. in May of that year; and some solar and spectroscopic work was also done in Europe.

The important fact of the possibility of thus getting pictures of the heavenly bodies was established, so that with the introduction of the collodion process in 1851, with its great advantages over the difficult and costly daguerreotype, astronomical photography was taken up and soon became firmly established. From this time its history became a record of continual advance, delayed, it is true, from time to time by the want of improvement in instrument or method, when further extensions of the art were attempted, in every case with ultimate success.

Of the early workers with the collodion process, and the more recent workers with the modern gelatine or dry plate process, and the persevering and skilful way they have dealt with the difficulties that always surround a new art, I do not propose to speak, except incidentally. Time would not allow me to do so here, nor is it part of my purpose; my object being rather to deal with the results obtained, to speak of this new art or method of astronomical observation and record, than to give an account of the labours of those who have made it what it now is. I propose to exhibit by the electric lantern such specimens of early and recent work as I have been able to obtain for this purpose,* and from which I think you may form an idea as to the present value of photography as an aid to astronomy, and the probable greater aid it may in future become.

Before doing so, however, although the art of photography is now known to almost every one, I should like to say a few words about the three processes I have mentioned, the daguerreotype, the collodion, and the gelatine or dry plate process, and also to give very shortly a general idea of the instruments and methods in use by the astronomical photographer.

For an account of the daguerreotype one has to refer to the text works, as it is not now in general use. Shortly it may be described as the use of a polished surface of silver (usually supported by a thin backing of copper) that has been exposed to the action of iodine so as to form a film of sensitive iodide of silver, that may be acted upon by light, and afterwards developed by the vapour of mercury. Although not now used for astronomical work, it may be that owing to the intimate connection between the image formed by the silver compound and the backing that supports it, it will be found to have advantages in cases where delicate measurements are required, that the other processes may not have.

---

* For the loan of some of the photographs exhibited I am indebted to the kindness of Mr. Crookes, Dr. de la Rue, Mr. Lockyer, and Captain Abney, and also to the Brothers Henry of Paris for specimens of their recent work.

The collodion and gelatine (or dry plate) processes are so called from the nature of the medium that is used to carry the sensitive salts of silver during exposure to the action of light. The chief difference between them, beyond the greater sensitiveness of the latter, is that with the collodion plate the process of preparation, exposure, and development must be part of a continuous operation taken in due order and time, with all the necessary apparatus, and the chemicals in a proper state of efficiency, ready to hand, conditions not easy to attend to in astronomical photography, and when long exposures are to be given, hardly to be fulfilled.

With the gelatine process the plates can be prepared beforehand under the best conditions, and almost any time may elapse before use. The exposure of the plate may take place for any length of time, and the development made at any suitable time afterwards; all advantages for astronomical work that are obvious; and in addition, there is the important advantage of the greatest sensitiveness, on which, more than anything else, success so much depends when it becomes a question of dealing with a small amount of light. Each process has its particular advantages of which the specialist may avail himself; but the gelatine dry plate is far beyond the others for nearly every class of astronomical work.

With regard to the instruments and methods of work, while the main principles are adhered to, modifications have to be made to suit different classes of work. These will be briefly mentioned in speaking of the photographs. In all cases there must be an image of the object, and this image must fall exactly on the sensitive plate, and be kept there during exposure.

The image produced by a telescope reflecting or refracting is generally used direct; indeed, with the exception of the sun, where the large amount of light rendered enlargement advantageous, until quite recently the primary image was always used, it being thought that with the small amount of light generally at disposal it was better to get a picture thus and then enlarge it afterwards than to enlarge the first image and so increase the time of exposure and all the trouble that comes from atmospheric and instrumental tremors, and other causes.

We shall see how much has been gained by departing from this old plan and using an enlarged image when we come to examine the photographs of the planets.

In the method of working there is one important difference between that followed by the terrestrial and celestial photographer. Without exception, everything that the latter has to photograph is in continual apparent motion, owing to the rotation of the earth, and in some cases to the proper motion of the object itself. This necessitates the use of an equatorial mounting to carry the photographic apparatus, with clockwork to give it a regular motion in a direction contrary to that of the earth. Even then the difficulties of keeping the telescope moving for one or more hours without allowing deviation of the image on the sensitive plate of a $\frac{1}{1000}$ of an inch during this time, taxes very severely the powers of the observer; for, every such long exposure must be watched not only to correct irregularities of the clock, but other slight though important movements, due to change of refraction and other causes, which, if not immediately corrected, would spoil the picture. These mechanical difficulties, however, are not insurmountable, as will be seen from some of the photographs I shall show; and as instruments improve and workers gain experience they will become less.

There are many technical details of extreme interest to the worker, which it is hardly necessary to name to-night.

The light from the different celestial bodies varies greatly in intensity. Between that from our sun and that from the faintest nebula or star that can be seen, there is such an immense difference, that their relative amounts can hardly be expressed by figures.

Dr. Huggins estimates the light of the faintest nebula that can be seen with a moderately large instrument as equal to $\frac{1}{20000}$ of the light of a single standard candle viewed at a distance of a quarter of a mile, that is, that such a candle a quarter of a mile off is 20,000 times more brilliant than the nebula.

The astronomer, who deals with both, has therefore need of all his art to reduce the light in the one case to that suitable for his purpose; and to utilise every ray he can get in the other, regulating the exposure for the one to a minute fraction of a second, and extending for the other to hours.

Between these two extremes of light-giving power are comprised all other celestial objects.

For the purpose of convenience I will take the photographs, which I propose to show you, in the following order:—(1) those of the sun; (2) the moon; (3) the stars; (4) planets; (5) nebulæ; and (6) comets; giving in nearly every case an early photograph and a recent one for comparison; and, where I can, a specimen of the work of eye and hand that may be directly compared with a photograph of the same object.

With the sun there are two distinct phenomena to observe: (1) the physical aspect of his surface, with the remarkable spots and markings that are frequently visible; and (2) the wonderful prominences and corona that surrounds the sun and becomes visible when he is eclipsed.

The first important photograph of the surface of the sun seems to have been obtained by Dr. de la Rue, July 24th, 1861.

The photograph I show is one copied from a picture, itself a reproduction, without retouch, of the original negative.

(Photographs of the surface of the sun, by Janssen, were also shown.)

Berkowski, in the eclipse of the 28th July, 1851, took by the daguerreotype the first photograph showing the corona and prominences, and Dr. de la Rue in 1860 took the first good photographs of the prominences, and obtained traces of the corona, using the collodion process.

In 1869, Professor Stephen Alexander obtained at Ollumwa the first good photographs of the corona.

(Photographs of the corona and prominences, by General Tennant, Dr. de la Rue, and Captain Abney were shown.)

Since this time, photography has been used at every total eclipse that has been observed, with increasing success.

In speaking of these corona photographs, I must not forget to mention the important work of Dr. Huggins in photographing the uneclipsed corona. He himself has lately given an account of the methods he employs, and I have no doubt that under his skilful direction we shall see the same successful advances as those we can mark in every branch of astronomical photography; although it is a work which many of those who know the great difficulties to be encountered would hardly have dared to attempt.

Photographs of the moon are so easy to obtain that she has received more attention than any other celestial object; yet, strange to say, with less improvement, the pictures taken by Rutherford more than twenty years ago, not being yet surpassed.

Now, however, that we can safely enlarge the primary image without unduly prolonging the exposure we may soon expect photographs of portions of the moon that will be far beyond anything hitherto done, or possible, where the whole image is attempted.

[Photographs of the moon, by Mr. Crookes, Dr. de la Rue, and others, were shown.]

With the stars photography has recently been most successful.

Rutherford, in 1864, completed a photographic objective of 11¼ inches aperture and 14 feet focal length, with which he obtained some very fine photographs. Some of his remarks, written in 1864,* in connection with the future of astronomical photography, are so interesting at the present time that I will repeat them. "Since the completion of the photographic objective, but one night has occurred (the 6th of March) with a fine atmosphere, and on that occasion the instrument was occupied with the moon; so that as yet I have not tested its powers upon the close double stars, 2″ being the nearest pair it has been tried upon. This distance is quite manageable provided the stars are of nearly equal magnitude.

---

* 'American Journal of Science and Art,' 2nd series, vol. xxxix. p. 308.

"The power to obtain images of the ninth magnitude stars with so moderate an aperture promises to develop and increase the application of photography to the mapping of the sidereal heavens and in some measure to realise the hopes which have so long been deferred and disappointed.

"It would not be difficult to arrange a camera-box capable of exposing a surface sufficient to obtain a map of two degrees square, and with instruments of large aperture we may hope to reach much smaller stars than I have yet taken. There is also every probability that the chemistry of photography will be very much improved and more sensitive methods devised."

In the light of recent work these words are almost prophetic. The sensitive methods have been devised. and the result is that the anticipations formed by Rutherford in 1864 are in 1886 not only fulfilled but exceeded. It is in stellar photography that the astronomer will be most benefited by the immense saving of labour in making charts; a single plate taken in one hour showing in their proper relative place and in their relative photographic magnitudes, all the stars down to the fifteenth magnitude over an area of about six degrees—a result that it would be hardly possible to obtain by the usual method of eye observation and measure.

[Photographs of the stars round Altair, taken in 1883, of part of Orion, taken in 1884 by the speaker, and some of the recent plates of stars and double stars, by the Brothers Henry of Paris, were shown, with a companion plate of similar parts of the sky as shown on Argelander's maps, showing the enormous increase in the number of stars shown by one hour's exposure, in one case ten times as many stars being on the photographic plate as on the same area of the map.]

The planets Jupiter and Saturn were photographed by Dr. de la Rue and others in the early days of photography. I have not been able to obtain copies, or even a sight of any of these earlier ones, but I have some of my own work that will enable you to see the improvement that has been made since 1876.

[Photographs of Saturn from 1877 and of Jupiter from 1878 were shown, including some of the recent work of the Brothers Henry of Paris, showing the great advance obtained by the enlargement of the primary image.]

With nebulæ, although the work has been all done within the last few years, the results have been very satisfactory.

Dr. Draper, in 1880, obtained a very promising picture of the Orion nebula, and I was able in 1883 (after trials commencing in 1879) to get, with a three-foot reflector, some very fair photographs. A comparison with the last drawing will show the chief points of difference.

Other nebulæ have been photographed by myself and others, and the power of the photograph to portray these mysterious shapes has been thoroughly demonstrated.

In a recent photograph of the Pleiades, which the MM. Henry have taken, they have obtained a picture of a nebula near Maia that has not been seen before, and which has since been seen with the great telescope at Pulkowa in Russia—a telescope very much larger than that with which the photograph was taken.

On this plate part of the nebula near Merope is also shown.

[Photographs of Orion with exposures of from 1 to 80 minutes were shown, and also photographs of the drawings made by the Bonds, Lord Rosse, Trouvelot, and others, for comparison.]

Of comets, I have here copies of two of the photographs taken at the Cape of Good Hope, in 1882. These pictures may be called photographs of stars from the immense number that have impressed themselves on the plate.

To others as well as to myself, these photographs came as a revelation of the power of photography in this direction, and it is probably to them that the increased attention lately given to stellar photography is due.

There are other applications of photography to the work of the astronomer besides those I have mentioned.

By the analysis of the light that comes to us from the heavenly bodies the spectroscope tells us what elementary substances exist in those bodies and in this most delicate research photography has played a most important part, especially in dealing with that part of the spectrum that the eye is not able to grasp.

The fleeting image that requires all the care that the mind can give to interpret is recorded by means of photography, and can then be studied under the most favourable conditions.

Such photographs cannot be shown in the same way as those I have shown you to-night, nor can they be rendered intelligible except to the very few who have made the consideration their study.

The recording of the passage of a star past the wires of a transit instrument has always hitherto been done by the eye, but it is quite possible that here photography may come in for this purpose, and render such observations free from personal equation, as the allowance that has to be made for different powers of different brains to record an event is called by astronomers.

There is also a possibility that photography will be available to record the paths of meteors and thus aid in a research that is engaging more attention every day.

The discovery of minor planets by means of photography cannot be helped when such photographs of the heavens as those taken by the MM. Henry are produced consecutively, a comparison from time to time being sufficient to detect the displacement in position that they must undergo, and it is not too much to say that if Uranus had not been discovered by Herschel, and in consequence of the disagreement between the position this planet should have occupied, and those it did occupy owing to the attraction of Neptune, and the subsequent discovery of this planet by the entirely theoretical investigations of

Professors Adams and Leverrier, both would have been discovered eventually by photography. And if there is now, as some suppose there is—and there is nothing against such a supposition—another major planet beyond Neptune, it is most probable that it will be thus discovered.

In thus bringing before you all these wonderful things that can be done by photography I do not wish to imply that it is quite a new thing. Astronomers have known and valued it, but the immense step it has lately taken through the great sensitiveness of the dry plate process has not, I believe, been fully realised.

For many years the art of photography as applied to astronomy has remained very nearly in the state it was when Rutherford wrote those remarks I have quoted. At a bound it has gone far beyond anything that was expected from it, and bids fair to overturn a good deal of the practice that has hitherto existed among astronomers. I hope soon to see it recognised as the most potent agent of research and record that has ever been within the reach of the astronomer; so that the records that the future astronomer will use, will not be the written impressions of dead men's views, but veritable images of the different objects of the heavens recorded by themselves as they existed.

[A. A. C.]

GENERAL MONTHLY MEETING,

Monday, March 1, 1886.

Warren de la Rue, Esq. M.A. D.C.L. F.R.S. Manager and Vice-President, in the Chair.

John Abercrombie, Sen. M.D. F.R.C.P.
William Henry Barlow, Esq. F.R.S. M. Inst. C.E.
Alfred Carpmael, Esq.
Henry Doetsch, Esq.
John Piggin Fearfield, Esq.
R. Gent-Davis, Esq. M.P.
John Hopkinson, Esq. M.A. F.R.S.
John Inglis, Esq.
Major E. Cecil Johnson.
Mrs. T. C. Leitch.
Mrs. S. Joshua.
George Palmer, Esq.
Sir Ughtred Kay-Shuttleworth, Bart. M.P.
Silvanus P. Thompson, Esq.
Sir William Thomson, D.C.L. LL.D. F.R.S.
Walter Tomlinson, Esq. M.A.

were elected Members of the Royal Institution.

At a Meeting of the Managers held this day, the Actonian Prize of one hundred guineas was awarded to Professor G. G. Stokes, Pres. R.S. for his lectures on Light, in conformity with the Acton Endowment Trust Deed.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Report on Public Instruction in Bengal, 1884–5. fol. 1885.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quarta, Vol. V. No. 12. 8vo. And Disegni. fol. 1885.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. Vol. II. Fasc. 1. 8vo. 1885-6.
Astronomical Society, Royal—Monthly Notices, Vol. XLVI. No. 3. 1886.
Bankers, Institute of—Journal, Vol. VII. Part 2. 8vo. 1886.
British Architects, Royal Institute of—Proceedings, 1885–6, Nos. 8, 9. 4to.
Chemical Society—Journal for February, 1886. 8vo.
Chief Signal Officer, United States Army—Professional Papers of the Signal Service, Nos. 16 and 18. 4to. 1885.

Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. VI. Part 1. 8vo. 1886.
Editors—American Journal of Science for February, 1886. 8vo.
Analyst for February, 1886. 8vo.
Athenæum for February, 1886. 4to.
Chemical News for February, 1886. 4to.
Engineer for February, 1886. fol.
Horological Journal for February, 1886. 8vo.
Iron for February, 1886. 4to.
Nature for February, 1886. 4to.
Revue Scientifique for February, 1886.
Science Monthly, Illustrated, for February, 1886. 8vo.
Telegraphic Journal for February, 1886. 8vo.
Ellis, Alexander John, Esq. B.A. F.R.S. M.R.I. (the Author)—Sensations of Tone. By H. L. F. Helmholtz. 2nd English edition. 8vo. 1885.
Musical Scales of Various Nations. (Journ. of Soc. of Arts.) 8vo. 1885.
Franklin Institute—Journal, No. 722. 8vo. 1886.
Geographical Society, Royal—Proceedings, New Series, Vol. VIII. No. 2. 8vo. 1886.
Geological Institute, Imperial, Vienna—Jahrbuch: Band XXXV. Heft 4. 8vo. 1885.
Geological Society—Quarterly Journal, No. 165. 8vo. 1886.
Geological Society of Ireland, Royal—Journal, Vol. XVI. Part 3. 8vo. 1886.
Iron and Steel Institute—Journal, 1885. Part 2. 8vo.
Johns Hopkins University—University Circular, No. 46. 4to. 1886.
Studies in Historical and Political Science, Fourth Series, No. 1. 8vo. 1886.
Mann, Robert James, M.D. F.R.C.S. M.R.I.—Lightning Conductors. By R. Anderson. 3rd edition. 8vo. 1885.
Marvin, C. Esq. (the Author)—Russia's Power of Attacking India. 8vo. 1886.
Maryland Medical and Chirurgical Faculty—Transactions, 87th Session. 8vo. 1885.
Meteorological Office—Report of Meteorological Council, R. S. to 31st March, 1885. 8vo. 1886.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXV. Part 1. 8vo. 1886.
Odontological Society of Great Britain—Transactions, Vol. XVIII. No. 4. New Series. 8vo. 1886.
Pharmaceutical Society of Great Britain—Journal, February, 1886. 8vo.
Photographic Society—Journal, New Series, Vol. X. No. 4. 8vo. 1886.
Preussische Akademie der Wissenschaften—Sitzungsberichte XL.-LII. 8vo. 1885-6.
Richardson, B. W. M.D. F.R.S. (the Author)—The Asclepiad, Vol. III. No. 9. 8vo. 1886.
Royal Society of London—Proceedings, No. 240. 8vo. 1885.
Scottish Society of Arts, Royal—Transactions, Vol. XI. Part 3. 8vo. 1885.
Smithsonian Institution—Third Report of the Bureau of Ethnology, 1881-2. 4to. 1884.
Society of Arts—Journal, February, 1886. 8vo.
Statistical Society—Journal, Vol. XLVIII. Part 4. 8vo. 1885.
Jubilee Volume, June, 1885. 8vo.
St. Bartholomew's Hospital—Reports, Vol. XXI. 8vo. 1885.
St. Pétersbourg, Academie des Sciences—Mémoires, Tome XXXIII. Nos. 3, 4. 4to. 1885.
United Service Institution, Royal—Journal, No. 132. 8vo. 1886.
University College, North Wales—Calendar, 1885-6. 12mo.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1886: Heft 1. 4to.

WEEKLY EVENING MEETING,

Friday, March 5, 1886.

Sir Frederick Bramwell, F.R.S. Honorary Secretary and Vice-President, in the Chair.

Professor Alexander Macalister, M.D. F.R.S.

Anatomical and Medical Knowledge of Ancient Egypt (*Abstract*).

The surviving fragments of the early literature of Egypt are mainly of a religious character; but this is not to be wondered at, for the genius of the people was essentially religious, and their doctrine of the future state leavened their national life in almost every particular; to them the body was an integral part of the immortal humanity, therefore it must not be permitted to turn to decay, but it must be preserved from corruption that it may be a fit receptacle for the soul to dwell in through eternity.

Their treatment of the body is thus dependent on their belief of its relation to the soul, and this, we learn from their religious writings, was a relationship of eternal interdependence.

To secure perpetual preservation the body must be properly embalmed. The cavities must be opened and subjected to the action of antiseptics. Even though the body is sacred, under the special protection of the god Thoth, though each part is under the guardianship of a special divinity, yet this sacredness does not preclude careful inspection and the processes necessary for preservation, for all parts have to be perpetuated.

Embalming is a religious rite, to be performed by the priests of the cultus, and the historian Herodotus has preserved for us what is doubtless a substantially accurate account of the different methods whereby it was done, in the later times in which he lived.

There is, in Diodorus Siculus, an account of an episode in the operation of embalming, which has long passed current as authentic. He says that when the sacred scribe inspected the body, he marked on its left side how much it was lawful to cut. Then the paraschistes, holding in his hand a knife of Ethiopian stone, dissected the flesh as far as the law permitted; then, turning suddenly, he fled away as fast as he was able to run, pursued by the execrations of the bystanders, often pelted with stones and otherwise maltreated. Then the taricheutæ enter, and passing their hands through the incisions into the body, remove therefrom the digestive organs, the heart, the kidneys, and other viscera. Then they wash out the cavities with palm wine and aromatics, and finally replace the parts which have been anointed with antiseptics.

While some portions of this description agree both with the earlier account given by Herodotus, and with the national literature, yet the first part I believe to be purely fictitious. There is no confirmation in Egyptian literature of that portion which relates to the ill-treatment of the outcast paraschistes. There is no trace of any such popular commotion in any of the pictures representing the stages of the process. Nay, we have direct testimony on the subject, for it is written in the Rhind papyrus, concerning the embalming of the Lady Ta-ani, " they made the incisions by the hand of a χar-heb in the place of opening, at the eighth hour." It was this grade of priests who began and who carried out the work of embalming. M. Revillout has proved conclusively that the *paraschistes* and *taricheutæ* are two Greek names for the same functionaries, whose native name is χar-heb. In the Rhind papyrus before quoted, it is written, " preserved is the body by the χar-heb, who is thoroughly acquainted with the science of embalming." From other passages in Egyptian writing we learn that these χar-hebs were priests, sacred scribes, men of high character, physicians, to whom even magical powers are ascribed, for the breath from the mouth of a χar-heb has power to dissipate disease of the heart. Further, we learn from several records that the incisions were not all made at once, but in at least two series; for the perfect preservation of a mummy was accomplished gradually, the process spreading over seventy days.

The χar-hebs were men with no civil disabilities; they bought and sold land, they made contracts, they drew up formal marriage settlements for their wives, and received payment in money, vegetables, wine, and other articles.

The organs removed from the bodies of persons of the better classes were not returned into the body, but were preserved in vases of alabaster or stone, surmounted by the heads of the four divinities of Hades, the sons of Horus and Isis. The vessel which contained the stomach and colon bore the human head of Amset; that which held the lungs and heart was under the protection of the jackal-head of Tuaut-mutef; that which contains the small intestine was beneath the baboon-head of Hapi; while the liver was guarded by the hawk-head of Kabhsenuf.

During the ascendancy of Greek influence in Egypt, Alexandria earned the reputation of being the chief school of anatomy and medicine in the world. Erasistratus, who lived in the days of Ptolemy Soter, B.C. 285, was an anatomist of such enthusiasm, that he and his disciples receiving from the king criminals condemned to death, " *vivos inciderint considerarintque etiam, spiritu remanente, ea quæ natura ante clausisset, eorumque posituram, colorem, figuram, magnitudinem, ordinem, duritiem, mollitiem, lævorem, contactum, processus deinde singulorum et recessus et sive quid inseritur alteri sive quid partem alterius in se recipit.*" It is recorded of Herophilus, the successor of Erasistratus, by Tertullian, that he dissected 600 bodies.

But this Alexandrian school, although upon Egyptian soil, was essentially Greek in spirit; even Herophilus had learned some of his anatomy from Praxagoras of Cos, although, as the anatomy of the earlier Greek school was originally derived from Egypt, it was but returning to the mother-country the traditions of culture derived therefrom. It was in Egypt Democritus of Abdera studied, and so was fitted to teach anatomy to Hippocrates, the father of medicine. The three pithy and graphic letters on anatomy which are extant, which it is supposed Democritus sent to Hippocrates, may well have been the result of his Egyptian training. At a later period, it was at Alexandria that Galen pursued his study of anatomy under Heraclianus, and the anatomical school of Alexandria survived until the Mohammedan invasion of Amru in A.D. 640. That much even of the earlier Greek medicine, anatomy, and pathology was derived from Egypt, we learn, both directly and indirectly. Most of the vegetable drugs in use in Greece were natives of Egypt; and Galen, speaking of one prescription called Epigonos, tells us that it was obtained from the adytum of the temple of Ptah, at Memphis. He quotes it, and other Egyptian prescriptions from the book Narthex, written by Hera of Kappadokia.

Medical colleges of far greater antiquity than that of Alexandria existed in the priestly schools of Memphis, Heliopolis, Sais, and Thebes. These were much more faithful exponents of the purely Egyptian system of the art of physic.

It was natural that Memphis should have been the centre of medical training in Egypt, as Imhotep, the patron god of medicine, was son of Ptah the creator, the great god of the Memphite triad. Here was the great library wherein students of the medical priesthood could learn their traditional lore. Even as late as the time of Clement of Alexandria, we read of the "*Tesserakonta ai panu anagkaiai tō Hermē gegonasi Biblioi*," which these pastophori studied; and of these, six were purely medical books which were respectively on Anatomy, on Disease, on Drugs, on Ophthalmology, on Surgical Instruments, and on Gynæcology. It is interesting to note that Clement gives to Anatomy the first place in the curriculum, as the foundation of medicine. Possibly the treatise Ambres, whose title has been preserved by Horapollo, may have been one of these. It was a sacred book whose rules were used by the physicians to make their prognosis, whether a patient was capable of recovery or no. It is probably from the title of this book that Hesychius has framed the verb Ambrizein, which he defines as *Therapeuein en tois ierois.* I cannot, however, find this verb in use by any author, and it has not obtained a place in Liddell and Scott. Such a book might well be the *peri Nosōn* of Clement. The name is strictly not a proper one, but is the Greek transliteration of the first words of one of these medical works.

Scant remains are left of these once famous seats of learning. Of the city of Memphis itself nothing remains but heaps which can scarcely be dignified as ruins, and of its medical library only a few fragments have been preserved.

Of the ancient medical literature of Egypt, two nearly complete treatises are still extant, and six or seven fragments of others. These vary in date and in perfection. The most complete are the Papyrus Ebers, and the Medical Papyrus of Berlin. The fragments which are noteworthy are:—The British Museum Papyrus, formerly the property of the Royal Institution, the Papyrus VI. of Boulaq, the Magical Papyri of Turin and Paris, the Coptic Medical Manuscript in the Borgia Library, and the Greek Papyri 383 and 384 of Leyden.

In the very brief sketch which the time at our disposal allows of the contents of these works, I cannot enter into the analysis of all these works, nor into discussion of disputed points of translation, but I hope soon to be in a position to publish a detailed and critical study of this medical literature at fuller length. We shall confine ourselves principally in our survey to the first and second.

The most complete of all these papyri is that which is known by the name Papyrus Ebers, said to have come from a tomb at El Assassif; and this is not improbable, as the Medical Papyrus VI. of Boulaq is from that place. It is in good preservation, written in a clear hieratic script, the characters resemble those of the earlier manuscript of the 18th dynasty; and its text presents few difficulties to the reader except those arising from the difficulty of ascertaining the precise diseases referred to, and the exact nature of the remedies prescribed. Unlike the Berlin Papyrus, it is perfect at its beginning, and it consists of 110 pages, each of twenty-one or twenty-two lines. A fac-simile copy has been published by Prof. Ebers, with a short synopsis of his reading of the contents. A brief notice of its contents has been published by Chabas, and it has partly been translated into Norwegian by Prof. Lieblein, but no complete translation has as yet been made public.

The work is really a series of treatises on different branches of medicine, and from its introductory paragraph seems to be the embodiment of Heliopolitan medical lore, probably dating from about 1550 B.C.

The first page, and the first five lines of the second, consist of an introductory preface and prayer:—"Beginning of the treatise on the administration of medicine to all parts of a person. I have come from Heliopolis, from the authorities of the great Temple, from the rulers of thought, the eternal governors of safety. I come from Sais with the mother goddesses as a protector to me; I speak for them; I do it from the Lord of the assembly conquering evil, the god slaying the slayers, whose several sections are from this my head, from this my neck, from these my arms, from these my limbs, from these my organs, to destroy the magical power of the ruler who influences my flesh, who sickens in these my limbs, and penetrates into my flesh, into my head, into my arms, into my body," &c.

After this prefatory adjuration follows a section on hygienic measures, cathartics, diuretics, and other remedies of the kind, which occupies the following sixteen pages. This is followed by a short section on the effects of the parasite Bilharzia hæmatobia, still a very common cause of disease in the Nile valley, and the pages from this to the twenty-third are taken up with prescriptions for parasites. Three prescriptions follow for uχet and others for uhau, whose treatment takes up the following four pages. Diseases of the colon, rectum, and heart are next prescribed for, and diseases of the head follow; one prescription is given as a quotation from another medical treatise, described as "the Old Scripture of the Wisdom of Men." Renal and gastric diseases follow, and a long section on ophthalmology, beginning, "Treatise on the sufferings in the eye, means for the cure of inflammation, and determination of blood to the eye." Diseases of the eye appear to have been as rife in Egypt then as they are still, and the author names twenty-two distinct diseases, some of which are briefly and graphically characterised. We can recognise among these, conjunctival inflammation, choroiditis, amaurosis, cataract, opacities of the cornea, as well as fistula lachrymalis, and others not so easily identified. One ointment for the eye, whose formula he gives, was invented by Chui, the president of the college; another is a foreign prescription in use among the Phœnicians at Byblus.

Nor were serious maladies alone attended to, but those which interfere with the appearance also claimed the physician's care. Twenty-four prescriptions of hair-washes, oils, depilatories, and dyes, are given, and some of them are characterised as very good. One of these prescriptions, found on p. 66, line 15, is entitled, "Another prescription to stimulate the growth of hair, prepared for the Lady Sesh, the mother of his Majesty King of Upper and Lower Egypt, Teta the blessed." This paragraph carries us back to the beginning of time, for Teta was the second king of the first dynasty, about 4000 B.C. The Egyptian priest Manetho notes in regard to him that he wrote books on human anatomy, for he was a physician.

After several smaller sections concerning diseases of the liver, there begins on p. 70 a treatise on the surgery of wounds, ulcers, erysipelas, cutaneous diseases, and diseases of the ear, nose, &c.

The section of this papyrus, which begins on the ninety-ninth page, is a treatise on the vascular system, entitled "The beginning of the mystery of medicine, knowledge of the motions of the heart, and knowledge of the heart." "There are vessels from it to all parts, which the physician Nebseχt, priest and Lord of Healing, describes. All these he points out with his fingers to the head, to the neck, to the hand, to the epigastrium, to the arms, to the legs; all he enumerates from the heart, because the vessels are from it to all parts; as he describes, it is the beginning of the vessels to each organ."

The author then proceeds to enumerate the distribution of these vessels from the heart, and first gives those ascending to the head.

Digressing from the vessels to the animal spirits, Nebseχt tells us that these vital spirits enter one nostril, penetrate to the heart through the tube which carries them into the body-cavity. A little farther on he states another singular hypothesis concerning these vital spirits; for, speaking of the ears, he says, "There are four vessels going to the two ears together, two on the right side, two on the left side, carrying the vital spirit into the one right ear, the breath of death into the left ear, that is, it enters on the right-hand side, the breath of death enters on the left-hand side."

Nebseχt next describes the vessels of the upper and lower limbs, and the arteries of the viscera.

The anatomical description is followed by a series of aphorisms regarding the pathology of vascular disease, arranged in separate sentences; protasis and apodosis beginning respectively with *Ar* and *pu*, reminding us of the *ην με* of Hippocrates; indeed, there is such a Hippocratic aspect about these that one cannot resist the conviction that we have reached here the source of much of the Hippocratic learning. It is possible that the earlier phrase may be interrogative, and the latter an answer; but it is more likely that the protasis is conditional than interrogative. They relate to such conditions as syncope, cardiac disturbance from abdominal distension, enlargement of the heart, pericardiac adhesion and effusion, dilatation, and enlargement of the heart, &c. There are twenty-two such queries, and some of them point to careful pathological observation; thus there is an allusion to valvular stenosis in one, which says, "If the orifice of the heart be turned back, then constricted is the mouth of the heart."

Of the Berlin papyrus a fac-simile has been published, and it has been discussed by three eminent scholars, Brugsch, Renouf, and Chabas. It is also written in hieratic characters, which are clear and legible, and was executed in the reign of Rameses II., the Pharaoh of the oppression, who reigned about 1300 B.C. The document consists of twenty-one leaves, two of which are written on both sides. The text is for the most part easily read; the chief difficulty presented by it, as by the other, is that of understanding the names of diseases and of remedies, to which no cognate words in Coptic, nor derivative words in other languages, have come down to us.

The other fragments are of literary rather than of medical interest. They abound in mystical formulæ, and seem to have been magical and occult rather than scientific. Of the Coptic MS. a transcript was published by Zoega, and it is evidently a fragment of a larger work; it deals chiefly with eruptive fevers and similar diseases.

[A. M.]

WEEKLY EVENING MEETING,

Friday, March 12, 1886.

THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. LL.D. President, in the Chair.

REGINALD STUART POOLE, Esq. LL.D. of the British Museum.

The Discovery of the Biblical Cities of Egypt.

BIBLICAL criticism has been pursued by three schools. The Old School was eminently conservative, and by too defensive an attitude had produced the strong reaction of the New School, whose tendency was in the opposite direction. Rejecting traditional authority, this school based all its arguments on the text itself. The appearance of the text, as redacted by the Masoretes in the sixth century of our era, seemed to it to favour the idea of the latest date in all cases, especially in that of the major part of the Pentateuch. The weak point in the researches of a body of scholars who had rendered the greatest services to verbal criticism was their failure to pay due consideration to historical evidence outside the record. Thus the Moabite Stone, a document of the ninth century before our era, reversed a canon of their criticism. Aramaic forms are now shown to be consistent with antiquity, and no longer a proof of late date. Again, existence of the Levitical body as generally understood, and the arrangement of the Levitical cities, is denied by Prof. Wellhausen; yet the list of the conquests of Shishak, Jeroboam's ally, enumerates ten Israelite cities, seven or eight of which belong to the Levitical list, thus explaining the statements of Chronicles that the Levites supported Rehoboam and were driven out by Jeroboam. This document, fully explained twenty-two years ago, was wholly ignored by Prof. Wellhausen. Such instances justified the existence of the Third or Historical School, which devotes itself to bringing all historical evidence to the elucidation of Biblical history. The chief weapon of this school is the spade of the excavator.

The Egypt Exploration Fund, desirous of finding monumental evidence of the history of the Hebrews in Egypt, succeeded, in 1883, in obtaining the services of Mr. Naville, one of the first living Egyptologists. On reaching Cairo, Mr. Naville asked the advice of Prof. Maspero, Director of the Museums and Excavations of Egypt. He was recommended to try the great mound of Tel-el-Maskhutah, on the Sweet-water Canal leading to Ismailia. This mound was supposed by Lepsius to cover the remains of Raamses or Rameses, one of the two store-cities built by the Israelites during the great oppression. Mr. Naville's excavation revealed not Rameses, but the sister store-city

Pithom. Pithom was at once a fortress and a place of stores, in extent not larger than Lincoln's Inn Fields, outside which a later town had grown. The walls of Pithom were 24 feet thick, of unburnt brick. A sixth of its area was found to be filled by store-chambers of massive construction, entered from above. Probably these occupied a still larger space, and there was also a temple built by Rameses II., founder of the town, a colossal hawk bearing whose name was brought thence by Mr. Naville, and is now in the British Museum. The bricks are also of the period of the same sovereign.

The first deduction from this evidence was the identification of Rameses II. as the great oppressor of the Hebrews. This view was started by Algernon, Duke of Northumberland, in Wilkinson's 'Ancient Egyptians' (i. p. 77 *seqq.*), with the modification that the first oppressor was Rameses I., and the founder of Pithom and Rameses was Rameses II. It was revived by Prof. Lepsius in the form first stated, and had already received the support of all leading Egyptologists. Equally Meneptah, the son of Rameses II., had been considered the Pharaoh of the Exodus. Their two reigns covered a period of eighty-six years, and as Rameses was already founded in the fifth year of the king who gave it his name, we had at least eighty-two years for the great oppression, to which the Bible allows at least eighty. The two Pharaohs correspond in the general portraiture of the Egyptian records and the lively descriptions of the Book of Exodus. Rameses is a proud, ruthless despot; Meneptah, with no less pretension, weak and vacillating. In consequence of this identification, the date of the Exodus in Egyptian evidence would be about B.C. 1320, and this date agrees with the most satisfactory scheme of Hebrew chronology, that of Lord Arthur Hervey (the Bishop of Bath and Wells), who, reckoning by the Hebrew genealogies, arrives at about the same date.

Pithom, the store-city, was so called by a sacred name Pi-tum, the abode of Tum, the setting sun; it had, as usual, a civil name also, Thekut, identified by Mr. Naville and Dr. Brugsch with Succoth, the second station of the Exodus, now one fixed point on the line of march. It was the centre of the Land of Succoth, each station being, as Mr. Naville contends, a country, not a place, a conclusion rendered necessary by the historical circumstances. The route was therefore along the Wadi-t-tumeylát, the narrow valley of the Sweet-water Canal.

In 1885 Mr. Naville had the good fortune to discover the town of Kesem, Goshen (LXX. Γεσέμ), near Zagazig, at the western extremity of the valley just mentioned. The position of the Land of Goshen was thus at last fixed as extending around this town and stretching southwards towards Heliopolis and eastwards to Pithom. The Land of Goshen is called in Scripture alternately the Land of Rameses, later known as the Arabian Nome. This Mr. Naville argues is due to the first organisation of this border-land into a settled district of Egypt by Rameses II., which led to the enslaving of the Hebrews as a necessary consequence. The town of Rameses as capital must have either been identical with the town of Goshen or close to it, and thus the eminent explorer has found the starting-point of the route of the Exodus.

It is important to observe that the religion of Goshen, as shown by a very important monument of Nectanebo, the last native Pharaoh, which is consistent with much earlier indications, yet far fuller in its extent, has no relation whatever to the Hebrew religion. The monument mentioned, a monolithic shrine, will be published in Mr. Naville's 'Memoir on Goshen,' the fourth annual volume of the Egypt Exploration Fund.

Mr. Naville's work was carried on by Mr. Petrie, and since by Mr. Griffith, at the site of Zoan, probably the capital of Joseph's Pharaoh. Amid a multitude of interesting discoveries made in this field, where, and in the adjacent country, the work is now being carried on, the most curious as bearing on the present subject was the discovery by Mr. Petrie, of the almost regal power of the viziers of the age of Joseph, one of whom put his name on a sphinx, a class of monument representing the king as a type of the sun, and elsewhere strictly limited to royal inscriptions.

Very curious light had been thrown on the obscure two centuries and a half, roughly, between the death of Joseph and the birth of Moses, when the Hebrews were subjugated rather than oppressed. Mr. Groff, a young Egyptologist of Paris, had recently identified two names of cities and tribes conquered by Thothmes III. B.C. cir. 1550, at the battle of Megiddo, fought against a great Canaanite and Syrian confederacy. These names are Jaakeb-el and Jeshep-el, which he held to be Jacob and Joseph in full form, as Nathaniel for Nathan.* If so, the Hebrews during this obscure period were engaged in border wars and even in military service abroad. This is consonant with the story of the death of Ephraim's sons in a border foray (1 Chron. vii. 20, 21), and the fact that the Israelites marched out of Egypt in battle array (Exod. xiii. 18).

Such were the consequences of historical criticism aided by discovery. They showed the essential antiquity of the part of Genesis and Exodus relating to the Hebrews in Egypt, and entitled the Egypt Exploration Fund to the sympathy of all lovers of research for the sake of truth.

[R. S. P.]

---

* See 'Rev. Égyptologique,' 1885, p. 95.

WEEKLY EVENING MEETING

Friday, March 19, 1886.

Sir William Bowman, Bart. LL.D. F.R.S. Manager and Vice-President, in the Chair.

W. H. M. Christie, Esq. M.A. F.R.S. Astronomer Royal.

Universal Time.

Considering the natural conservatism of mankind in the matter of time-reckoning it may seem rather a bold thing to propose such a radical change as is involved in the title of my discourse. But in the course of the hour allotted to me this evening, I hope to bring forward some arguments which may serve to show that the proposal is not by any means so revolutionary as might be imagined at the first blush.

A great change in the habits of the civilised world has taken place since the old days when the most rapid means of conveyance from place to place was the stage-coach, and minutes were of little importance. Each town or village then naturally kept its own time, which was regulated by the position of the sun in the sky. Sufficient accuracy for the ordinary purposes of village life could be obtained by means of the rather rude sun-dials which are still to be seen on country churches, and which served to keep the village clock in tolerable agreement with the sun. So long as the members of a community can be considered as stationary, the sun would naturally regulate, though in a rather imperfect way, the hours of labour and of sleep and the times for meals, which constitute the most important epochs in village life. But the sun does not really hold a very despotic sway over ordinary life, and his own movements are characterised by sundry irregularities to which a well-ordered clock refuses to conform.

Without entering into detailed explanation of the so-called "Equation of Time," it will be sufficient here to state that, through the varying velocity of the earth in her orbit, and the inclination of that orbit to the ecliptic, the time of apparent noon as indicated by the sun is at certain times of the year fast and at other times slow, as compared with 12 o'clock, or noon by the clock. [The clock is supposed to be an ideally perfect clock going uniformly throughout the year, the uniformity of its rate being tested by reference to the fixed stars.] In other words, the solar day, or the interval from one noon to the next by the sun, is at certain seasons of the year shorter than the average, and at others longer, and thus it comes about that by the accumulation of this error of going, the sun is at the beginning of November more than 16 minutes fast, and by the middle of February 14½ minutes slow, having lost 31 minutes, or more than half-an-hour, in the interval. In passing it may be mentioned as a result of this that the afternoons in November are about half-an-hour shorter than the mornings, whilst in February the mornings are half-an-hour shorter than the afternoons. In view of the importance attached by some astronomers to the use of exact local time in civil life, it would be interesting to know how many villagers have remarked this circumstance.

It is essential to bear these facts in mind when we have to consider the extent to which local time regulates the affairs of life, and the degree of sensitiveness of a community to a deviation of half-an-hour or more in the standard reckoning of time. My own experience is that in districts which are not within the influence of railways the clocks of neighbouring villages commonly differ by half-an-hour or more. The degree of exactitude in the measurement of local time in such cases may be inferred from the circumstance that a minute-hand is usually considered unnecessary. I have also found that in rural districts on the Continent arbitrary alterations of half-an-hour fast or slow are accepted not only without protest but with absolute indifference.

Even in this country where more importance is attached to accurate time, I have found it a common practice in outlying parts of Wales (where Greenwich time is about twenty minutes *fast* by local time) to keep the clock half-an-hour fast by railway (i.e. Greenwich) time, or about fifty minutes fast by local time. And the farmers appeared to find no difficulty in adapting their hours of labour and times of meals to a clock which at certain times of the year differed more than an hour from the sun.

There is a further irregularity about the sun's movements which makes him a very unsafe guide in any but tropical countries. He is given to indulging in a much larger amount of sleep in winter than is desirable for human beings who have to work for their living and cannot hibernate as some of the lower animals do. To make up for this he rises at an inconveniently early hour in summer and does not retire to rest till very late at night. Thus it would seem that a clock of steady habits would be better suited to the genius of mankind.

Persons whose employment requires daylight must necessarily modify their hours of labour according to the season of the year, whilst those who can work by artificial light are practically independent of the vagaries of the sun. Those who work in collieries, factories, or mines, would doubtless be unconscious of a difference of half-an-hour or more between the clock and the sun, whilst agriculturists would practically be unaffected by it, as they cannot have fixed hours of labour in any case.

Having thus considered the regulating influence of the sun on ordinary life within the limits of a small community, we must now take account of the effect of business intercourse between different communities separated by distances which may range from a few miles to half the circumference of our globe. So long as the means of communication were slow, the motion of the traveller was insignificant compared with that due to the rotation of the earth, which gives us our measure of time. But it is otherwise now, as I will proceed to explain.

Owing to the rotation of the earth about its axis, the room in which we now are is moving eastward at the rate of about 600 miles an hour. If we were in an express train going eastward at a speed of sixty miles an hour (relatively to places on the earth's surface), the velocity of the traveller due to the combined motions would be 660 miles an hour, whilst if the train were going westward it would be only 540 miles. In other words, if local time be kept at the stations, the apparent time occupied in travelling sixty miles eastward would be 54 minutes, whilst in going sixty miles westward it would be 66 minutes. Thus the journey from Paris to Berlin would apparently take an hour and a half longer than the return journey, supposing the speed of the train to be the same in both cases.

In Germany, under the influence of certain astronomers, the system of local time has been developed to the extent of placing posts along the railways to mark out each minute of difference of time from Berlin. Thus there is an alteration of one minute in time reckoning for every ten miles eastward or westward, and even with the low rate of speed of German trains, this can hardly be an unimportant quantity for the engine-drivers and guards, who would find that their watches appeared to lose or gain (by the station clocks) one minute for every ten miles they have travelled east or west. This would seem to be the *reductio ad absurdum* of local time.

In this country the difficulty as to the time-reckoning to be used on railways was readily overcome by the adoption of Greenwich time throughout Great Britain. The railways carried London (i. e. Greenwich) time all over the country, and thus local time was gradually displaced. The public soon found that it was important to have correct railway time, and that even in the west of England, where local time is about 20 minutes behind Greenwich time, the discordance between the sun and the railway clock was of no practical consequence. It is true that for some years both the local and the railway times were shown on village clocks by means of two minute-hands, but the complication of a dual system of reckoning time naturally produced inconvenience, and local time was gradually dropped. Similarly in France, Austria, Hungary, Italy, Sweden, &c., uniform time has been carried by the railways throughout each country. It is noteworthy that in Sweden the time of the meridian one hour east of Greenwich has been adopted as the standard, and that local time at the extreme east of Sweden differs from the standard by about 36½ minutes.

But in countries of great extent in longitude, such as the United

States and Russia, the time-question was not so easily settled. It was in the United States and Canada that the complication of the numerous time standards then in use on the various railways forced attention to the matter. To Mr. Sandford Fleming, the constructor of the Inter-Colonial Railway of Canada and engineer-in-chief of the Pacific Railway, belongs the credit of having originated the idea of a universal time to be used all over the world. In 1879 Mr. Fleming set forth his views on time-reckoning in a remarkable paper read before the Canadian Institute. In this he proposed the adoption of a universal day, commencing at Greenwich mean noon or at midnight of a place on the anti-meridian of Greenwich, i. e. in longitude 180° from Greenwich. The universal day thus proposed would coincide with the Greenwich astronomical day, instead of with the Greenwich civil day which is adopted for general use in this country.

The American Metrological Society in the following year issued a report recommending that, as a provisional measure, the railways in the United States and Canada should use only five standard times, 4, 5, 6, 7, and 8 hours respectively later than Greenwich, a suggestion originally made in 1875 by Prof. Benjamin Pierce. This was proposed as an improvement on the then existing state of affairs, when no fewer than seventy-five different local times were in use on the railroads, many of them not differing more than 1 or 2 minutes. But the committee regarded this merely as a step towards unification, and they urged that eventually one common standard should be used as railroad and telegraph time throughout the North American continent, this national standard being the time of the meridian 6 hours west of Greenwich, so that North American time would be exactly 6 hours later than Greenwich time.

Thanks to the exertions of Mr. W. F. Allen, Secretary of the General Railway Time Convention, the first great practical step towards the unification of time was taken by the managers of the American railways on November 18, 1883, when the five time standards above mentioned were adopted. Mr. Allen stated in October 1884, that these times were already used on 97½ per cent. of all the miles of railway lines, and that nearly 85 per cent. of the total number of towns in the United States of over 10,000 inhabitants had adopted them.

I wish to call particular attention to the breadth of view thus evinced by the managers of the American railways. By adopting a national meridian as the basis of their time-system, they might have rendered impracticable the idea of a universal time to be used by Europe as well as America. But they rose above national jealousies, and decided to have their time-reckoning based on the meridian which was likely to suit the convenience of the greatest number, thus doing their utmost to promote uniformity of time throughout the world by setting an example of the sacrifice of human susceptibilities to general expediency.

Meanwhile Mr. Sandford Fleming's proposal had been discussed at the Geographical Congress at Venice in 1881, and at a meeting of the Geodetic Association at Rome in 1883. Following on this a special Conference was held at Washington in October 1884, to fix on a meridian proper to be employed as a common zero of longitude and standard of time-reckoning throughout the globe. As the result of the deliberation it was decided to recommend the adoption of the meridian of Greenwich as the zero of longitude, and the Greenwich civil day (commencing at Greenwich midnight and reckoned from 0 to 24 hours) as the standard for time-reckoning. In making this selection the delegates were influenced by the consideration that the meridian of Greenwich was already used by an overwhelming majority of sailors of all nations, being adopted for purposes of navigation by the United States, Germany, Austria, Italy, &c. Further, the United States had recently adopted Greenwich as the basis of their time-reckoning, and this circumstance in itself indicated that this was the only meridian on which the Eastern and Western Hemispheres were likely to agree.

The difficulties in the way of an agreement between the two hemispheres may be appreciated by the remarks of the Superintendent of the American Ephemeris on Mr. Sandford Fleming's scheme for universal time (which was subsequently adopted in its essentials at the Washington Conference):—"A capital plan for use during the millennium. Too perfect for the present state of humanity. See no more reason for considering Europe in the matter than for considering the inhabitants of the planet Mars. No; we don't care for other nations, can't help them, and they can't help us." *

As a means of introducing universal time, it has been proposed by Mr. Sandford Fleming, Mr. W. F. Allen, and others, that standard times based on meridians differing by an exact number of hours from Greenwich should be used all over the world. In some cases it may be that a meridian differing by an exact number of half-hours from Greenwich would be more suitable for a country like Ireland, Switzerland, Greece, or New Zealand, through the middle of which such a meridian would pass, whilst one of the hourly meridians would lie altogether outside of it.

The scheme of hourly meridians, though valuable as a step towards uniform time, can only be considered a provisional arrangement, and though it may work well in countries like England, France, Italy, Austria, Hungary, Sweden, &c., which do not extend over more than one hour of longitude, in the case of such an extensive territory as the United States difficulties arise in the transition from one hour-section to the next which are only less annoying than those formerly experienced, because the number of transitions has been reduced from seventy-five to five, and the change of time has been made so large that there is less risk of its being overlooked. The natural inference from this is that one time-reckoning should be used throughout the

---

* 'Proceedings' of the Canadian Institute, Toronto, No. 143, July 1885.

whole country, and thus we are led to look forward to the adoption in the near future of a national standard time, six hours slow by Greenwich, for railways and telegraphs throughout North America.

We may then naturally expect that by the same process which we have witnessed in England, France, Italy, Sweden, and other countries, railway time will eventually regulate all the affairs of ordinary life. There may of course be legal difficulties arising from the change of time-reckoning, and probably in the first instance local time would be held to be the legal time unless otherwise specified.

It seems certain that when a single standard of time has been adopted by the railways throughout such a large tract of country as North America, where we have a difference of local times exceeding five hours, the transition to universal time will be but a small step.

But it is when we come to consider the influence of telegraphs on business life, an influence which is constantly exercised, and which is year by year increasing, that the necessity for a universal or world time becomes even more apparent. As far as railways are concerned, each country has its own system, which is to a certain extent complete in itself, though even in the case of railways the rapidly increasing inter-communication between different countries makes the transition in time-reckoning on crossing the frontier more and more inconvenient. Telegraphs, however, take no account of the time kept in the countries through which they pass, and the question, as far as they are concerned, resolves itself into the selection of that system of time-reckoning which will give least trouble to those who use them.

For the time which is thus proposed for eventual adoption throughout the world, various names have been suggested. But whether we call it Universal, Cosmic, Terrestrial, or what seems to me best of all, World Time, I think we may look forward to its adoption for many purposes of life in the near future.

The question, however, arises as to the starting-point for the universal or world day. Assuming that, as decided by the great majority of the delegates at Washington, it is to be based on the meridian of Greenwich, it has still to be settled whether the world day is to begin at midnight or noon of that meridian. The astronomers at Rome decided by a majority of twenty-two to eight in favour of the day commencing at Greenwich noon, that is, of making the day throughout Europe begin about midday. However natural it might be for a body of astronomers to propose that their own peculiar and rather inconvenient time-reckoning should be imposed on the general public, it seems safe to predict that a World Day which *commenced* in the middle of their busiest hours would not be accepted by business men. In fact, the idea on which this proposal was founded was that universal time would be used solely for the internal administration of railways and telegraphs, and that accurate local time must be rigidly adhered to for all other purposes. It was conceded, however, that persons who travelled frequently might with advantage use universal time during railway journeys. This attempt to separate the travelling from the stationary public seems to be one that is not likely to meet with success even temporarily, and it is clear that in the future we may expect the latter class to be completely absorbed in the former. Another argument that influenced the meeting at Rome was the supposed use of the astronomical day by sailors. Now it appears that sailors never did use the astronomical day, which begins at the noon *following* the civil midnight of that date, but the nautical day which begins at the noon *preceding*, i. e. twenty-four hours before the astronomical day of the same date, ending when the latter begins. And the nautical day itself has long been given up by English and American sailors, who now use a sort of mongrel time-reckoning, employing civil time in the log-book and for ordinary purposes, whilst, in working up the observations on which the safe navigation of the ship depends, they are obliged to change civil into astronomical reckoning, altering the date where necessary, and interpreting their a.m. and p.m. by the light of nature. It says something for the common-sense of our sailors that they are able to carry out every day without mistake this operation, which is considered so troublesome by some astronomers.

In this connection I may mention that the Board of Visitors of Greenwich Observatory have almost unanimously recommended that, in accordance with the resolution of the Washington Conference, the day in the English 'Nautical Almanac' should be arranged from the year 1891 (the earliest practicable date) to begin at Greenwich midnight (so as to agree with civil reckoning, and remove this source of confusion for sailors), and that a committee appointed by them have drawn up the details of the changes necessary to give effect to this resolution without causing inconvenience to the mercantile marine.

The advantage of making the world day coincide with the Greenwich civil day is that the change of date at the commencement of a new day falls in the hours of the night throughout Europe, Africa, and Asia, and that it does not occur in the ordinary office hours (10 a.m. to 4 p.m.) in any important country except New Zealand. In the United States and Canada the change of date would occur after four in the evening, and in Australia before ten in the morning. This arrangement would thus reduce the inconvenience to a minimum, as the part of the world in which the change of date would occur about the middle of the local day is almost entirely water, whilst on the opposite side we have the most populous continents.

The question for the future seems to be whether it will be found more troublesome to change the hours for labour, sleep, and meals once for all in any particular place, or to be continually changing them in communications from place to place, whether by railway, telegraph, or telephone. When universal or world time is used for railways and telegraphs, it seems not unlikely that the public may find it more convenient to adopt it for all purposes. A business man who daily travels by rail, and constantly receives telegrams from all parts of the world, dated in universal time, would probably find it easier to learn once for all that local noon is represented by 17h. U.T. and midnight by 5h. (as would be the case in the Eastern States of North America), and that his office hours are 15h. to 21h. U.T., than to be continually translating the universal time used for his telegrams into local time.

If this change were to come about, the terms noon and midnight would still preserve their present meaning with reference to local time, and the position of the sun in the sky, but they would cease to be inseparably associated with twelve o'clock.

The introduction of Universal Time would practically involve the adoption of the system of counting the hours in one series from 0 to 24, instead of in the two series 0 to 12 a.m. and p.m., for as applied to Universal Time the terms *ante-meridiem* and *post-meridiem* would be meaningless, except for places on the meridian of Greenwich. The use of the 24 hour system on railways and telegraphs would naturally assist in breaking the spell of habit which associates noon and midnight with 12 o'clock.

It may be mentioned that the Eastern and Eastern Extension Telegraph Companies already use the 24 hour system throughout their extensive lines of telegraph to avoid mistakes of a.m. and p.m., and to save telegraphing these unnecessary letters. In this connection the President of the Western Union Telegraph Company in the United States has stated that the adoption of the 24 hour mode of reckoning would, besides materially reducing the risk of error, save at least 150 million letters annually on the lines of his company. It is also noteworthy that 98 per cent. of the railway managers in the United States, representing 60,000 miles of railway, have expressed themselves in favour of the adoption of the simple notation from 0 to 24 hours.

Considering that the only change which we are called on, in accordance with the Washington Resolutions, to make in our time-reckoning on railways is the adoption of the 24 hour system, it may be hoped that our railway companies will not be behind those of the United States in appreciating the simplification in railway time-tables, which would result from this reform.

[W. H. M. C.]

WEEKLY EVENING MEETING,

Friday, March 26, 1886.

SIR WILLIAM BOWMAN, Bart. LL.D. F.R.S. Vice-President, in the Chair.

PROFESSOR W. CHANDLER ROBERTS-AUSTEN, F.R.S. *M.R.I.*

CHEMIST OF THE MINT, PROFESSOR OF METALLURGY, NORMAL SCHOOL OF SCIENCE AND ROYAL SCHOOL OF MINES.

On Certain Properties common to Fluids and Solid Metals.

IN one of the beautiful discourses, delivered in the early part of the last century, which grace the annals of the French Academy of Sciences,* Réaumur observes that industrial art, like nature, has its marvels, which we often fail to notice because they are constantly before us.

The extraordinary ductility of metals appeared to him to involve one of the deepest secrets of nature, and although he held that in his time science was hardly in a position to explain more fully than the old philosophers did, the cause of this property of bodies, it was nevertheless possible to see better than they, what advantage art has gathered from the power of leading and guiding metals by hammering or by traction, and from this point of view, both art and nature seem, he says, to rival each other in furnishing us with remarkable facts. Réaumur then, with singular clearness, defines the conditions under which metals prove to be ductile. The relation between the behaviour of solid metals and fluids has long been recognised, not merely in the sense that atomic motion is common to solids and fluids, and that therefore "everything moves and nothing remains," but apart from theory there is much experimental evidence as to the properties that are common to fluids and solid metals, the characteristics of which, at first sight, seem widely separated. Let me remind you of the elementary definition of the two states, *solid* and *liquid.* A solid has a definite external form which either does not change, or only changes with extreme slowness when left to itself, and, in order to change this form rapidly, it is necessary to exert a more or less energetic effort. A liquid, on the other hand, can be said to have no form of its own, as it always assumes that of the containing vessel, the mobility of its particles is extreme, its resistance to rupture is very small, and its free surface is always a plane when the mass is left at rest. Then there is the colloid condition, which intervenes between the liquid and crystalline solid state, extending into both, and probably affecting all kinds of solid and liquid matter in a greater or less degree. The colloid or jelly-like body does present a certain amount of resistance to change of shape. Such a substance is well imitated by a skin of thin india-rubber filled with water. Another illustration is probably afforded by iron and other substances which soften under heat, and may be supposed to assume, at the same time, a colloid condition. Lastly, there is the gaseous condition of matter, with which we have but little to do at present.

---

* 'Histoire de l'Academie Royale des Sciences,' 1713 [vol. for 1739, p. 199].

We are in the habit of regarding metals as typical solids. I hope to trace this evening the analogies of their behaviour under certain conditions with that of fluids, and the following list shows the order in which I propose to group the properties common to fluids and solid metals:

1. Rejection of impurities on solidification.
2. Surfusion.
3. Flow under pressure.
4. Changes due to compression.
5. Absorption of gases.
6. „ liquids.
7. Diffusion.
8. Vaporisation.
9. Surface tension.

The transition from the liquid to the solid state is marked by the same phenomena in the case of many metals, as is observed in certain fluids, and I must dwell on this very briefly as leading up to the relations between solid metals and fluids, which come more definitely within the title of the lecture.

Water on passing from the liquid to the solid state undergoes a partial purification, the ice first formed being sensibly more free from colouring matter or suspended particles than the water from which it separates.

Many metals on freezing similarly eject impurities. In the case of alloys, saturated solutions, of one metal in another, appear to be formed, and excess of metal ejected, a fact which is being studied with much care by my colleague, Dr. Guthrie. The prominent facts are perhaps best illustrated by reference to a frozen mixture of copper, antimony, and lead. The results of some experiments conducted in my laboratory by my assistant, Dr. E. J. Ball, show that when a molten mixture of these metals is solidified, a definite atomic alloy of copper and antimony, which possesses a beautiful violet tint, first forms, and, after saturating itself with lead, up to a certain point, it ejects the rest of the lead, driving it to the centre of the mass so as to form a sharp line of demarcation, as is shown in the engraving, Fig. 1, the outer circle of which represents violet, and the inner grey, presenting a direct analogy to the ice, which is comparatively colourless, first forming from coloured water. Then there is another remarkable analogy between the freezing of certain fluids and the solidification of some metals. Water may, as is well known, be cooled down to −8° Cent., without solidification, but agitation immediately determines the formation of ice, and at the same time a thermometer plunged in the water rises to zero. Faraday stated, in 1858, that fused acetic acid, sulphur, phosphorus, many metals and many solutions,* would exhibit the same effect. Tin also may be cooled to several degrees below its solidifying point without actually freezing, and Dr. Van Riemsdijk,† of Utrecht, has observed that a globule of gold or silver, in a fused state, will pass below its solidifying point without actually solidifying, but the slightest touch with a metallic point will cause the metal to solidify, and the consequent release of its latent heat of fusion is sufficient to raise the globule to the melting point again, as is indicated by a brilliant glow which the button emits, a beautiful effect which I hope to show you. [The experiment was then made.]

FIG. 1.

It may be well also to remind you incidentally that a minute variation in composition is sometimes sufficient to lower the melting point of a metal or alloy, as is instanced by the addition of $\frac{2}{10}$ per cent. of silicon to standard gold, which, as you will observe in the case of this strip of the alloy, softens in the flame of a candle, or at about the melting point of zinc, 412° Cent., although the melting point of standard gold, free from silicon, would be over 1000° Cent.

Now to pass to solid metals. It is the common experience of us all, that a counterfeit shilling, consisting principally of lead, does not "ring" when thrown on a wooden surface. In 1726, Louis Lemery observed that lead is under certain conditions almost as sonorous as bell-metal.‡ He communicated the fact to Réaumur, who, being much struck by it, investigated the conditions under which lead becomes sonorous, and submitted the results to the French Academy.§ He pointed out that in describing a body which is not sonorous, it is usual to say that it is as "dull as lead," an expression which has become proverbial. "Nevertheless," he adds, "under certain conditions, lead has a property both novel and remarkable, for it emits surprisingly sharp notes when struck with another piece of lead." He showed that it was necessary that the lead should be formed by casting into a segment of a sphere, that is, mushroom shaped, as in the specimens of lead exhibited. The lead must be free from prominences, and must be neatly trimmed. The effect is less marked, if the lead be very pure, than if ordinary commercial lead be used, but it is only a question of degree.

---

* Faraday, 'Experimental Researches in Chemistry and Physics,' p. 379.

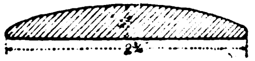
† Dr. Van Riemsdijk, 'Ann. de Chim. et de Phys.' t. xx. 1880, p. 66.

‡ Hoefer, 'Histoire de la Chimie,' t. ii. p. 374.

§ 'Histoire de l'Academie Royale des Sciences,' Anné 1726 [vol. for 1728, p. 243].

[A mass of pure lead cast into the shape shown in the sketch, Fig. 2, was struck with a piece of lead, and it emitted a sharp, clear note.]

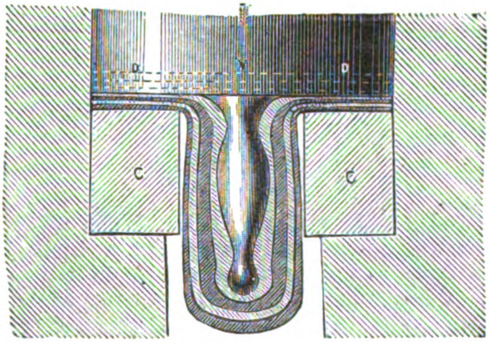
FIG. 2.



I have shown you the experiment mainly for the sake of being able to quote Réaumur's observations upon it. He showed that the sonorous lead might be rendered dull by hammering it. Here is lead from the same sample of metal as that from which the sonorous mass was cast, but it has been flattened out, and you will observe that it is "dull." I think his remarks have been overlooked in late years. He was led to the belief that in cast lead there must be an arrangement of the interior of the mass which the hammer cannot impart, because lead fashioned by hammering into the same form as the sonorous cast mass, is dull, and, more important still, he held that the fibrous and granular structure of the lead is modified in a manner which makes it probable that the sound is due to the shape of the grains, and to the "way in which they touch each other"; further, the blows of the hammer not only change the arrangement of the fibres, but they alter the shape of the grains, "the round grains are rendered flat, they are compelled to elongate and fill the interstitial spaces which previously existed between them. The particles are no longer free to vibrate, hence the lead is dull." These remarks derive additional interest, if we compare them with the observations in Professor Osborne Reynolds' most important lecture on "Dilatancy in Granular Matter" recently delivered here. We shall also, I think, see that this description of Réaumur's shows that he fully appreciated the theoretical importance of the kind of facts depending on the transfer of metallic matter from one position to another, which we now consider to be characteristic of the "flow" of metals; at any rate I have thought it well to make Lemery's experiment the starting point of the rest of the remarks I have to offer you.

A solid may be very brittle, and may yet, if time be given to it, flow from one point to another. This stick of sealing-wax was supported at its ends, and it has in a few weeks bent at the ordinary atmospheric temperature, although at any given point of its flow it would have been easy to snap it with a slight application of force. This much thinner strip of pure lead of the same breadth as the sealing-wax, also bends at the ordinary temperature with its own weight, the ends being supported. Sir William Thomson has pointed out that a gold wire sustaining half the weight which would actually break it, would probably not rupture in a thousand or even a million years, that is to say, there would be no "flow" ending in disruption; if, however, force be suitably applied, metals will flow readily. First, let us examine the case of a metal under force applied so as to compel it to flow through a hole, and I would point to the analogy of an ordinary viscous fluid. This vessel containing treacle is provided with a cylindrical hole in its base, and on the removal of the plug which now closes it, the treacle will flow out, the end of the stream being rounded. If a similar vessel be filled with lead it will, at the ordinary pressure, remain there, but if pressure be applied the lead will prove by its behaviour that it is really a viscous solid, as it flows readily through the orifice; the end of the jet is rounded, and, as has been shown by the beautiful researches of the late M. Tresca, of Paris,* all the molecules which compose the original block place themselves in the jet absolutely as the molecules of a flowing jet of a viscous fluid would. If the metal has a constant "head," as it would be termed in the case of water, that is, if the vessel be kept filled with solid lead up to a certain level, then you have a continuous stream, the length depending on the constancy with which the "head" and the pressure are maintained. If, on the other hand, the head is diminished so that nearly all the solid lead has been allowed to flow away, you have a folding of the jet, and vertical corrugations, exactly such as would characterise the end of the flow of certain other viscous fluids, and finally the jet forms a distinct funnel-shaped tube, concentric with the jet. It is also seen that when the formation of these cavities takes place, the jet is no longer equal to the full diameter of the orifice, as is shown in Fig. 3, the formation of the contracted vein is manifest, and a new analogy is thus obtained between the flow of solids and liquids. The application of this fact, that solid metals flow like viscous fluids, is of great importance in industry, and the

FIG. 3.

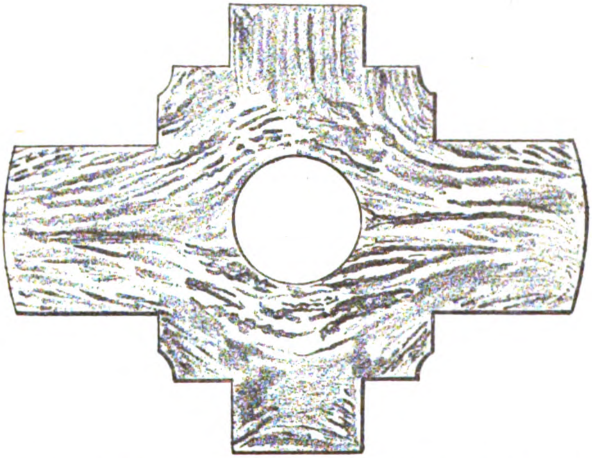


---

* These researches extend through a long series of memoirs; those relating to the flow of metals are well summarised in the 'Proc. Inst. Mech. Engineers,' 1867, p. 114, and in the report of the Science Conferences held in connection with the Loan Collection of Scientific Apparatus (Physics and Mechanics), London, 1876, p. 252.

production of complicated forms by forging or by rolling iron and steel and other metals, entirely depends on the flow of the metal when suitably guided by the artificer. The lines of flow in iron may be well shown by polishing a surface of the metal, and by submitting it to the action of a solution of bichloride of mercury, which etches the surface, or better, to the slow action of chromic acid solution, as suggested by Sir Frederick Abel, the result in either case being, that any difference in the hardness of the metal or in the chemical composition, or want of continuity, caused by the presence of traces of entangled slag, reveals the manner in which the metal has flowed. The engravings illustrate the direction of flow in the following cases. Fig. 4 is a section of a forged crosshead, kindly sent me by Mr. Webb of Crewe. Fig. 5 represents the top, planed and etched, of a hemisphere of mild steel, of the form and dimensions shown in the sectional view, Fig. 6, $\frac{5}{8}$ in. thick, "dished" cold by forcing a plate through a circular orifice. The convolutions of the etched portions afford evidence of the struggle sustained by the flowing particles of the metal. The experiments of M. Tresca were not made on "cinder-free" metal; it is therefore interesting to compare the etched section of the old rail, Fig. 7, the result of the complicated welding of puddled iron, with a basic-Bessemer rail, rolled from steel which has been cast, and which is therefore free from entangled slag. Fig. 8 represents a section of such a rail presented to me by Mr. P. C. Gilchrist.

FIG. 4.



A very striking illustration of the importance of the flow of metals, when used in construction, is afforded by some observations of Mr. B. Baker, in a recent paper on the Forth Bridge.* He says, "If the thing were practicable, what I should choose as the material for the compression members of a bridge, would be 34 to 37-ton steel, which had been previously squeezed endwise, in the direction of the stress, to a pressure of about 45 tons per square inch, the steel plates being held in suitable frames to prevent distortion." He adds, "My experiments have proved that 37-ton steel so treated will carry as a column as much load as 70-ton steel in the state in which it leaves the rolls, that is to say, not previously pressed endwise. . . . . At least one half of the 42,000 tons of steel in the Forth Bridge is in compression, and the same proportion holds good in most bridges, so the importance of gaining an increased resistance of 60 per cent. without any sacrifice

FIG. 5.

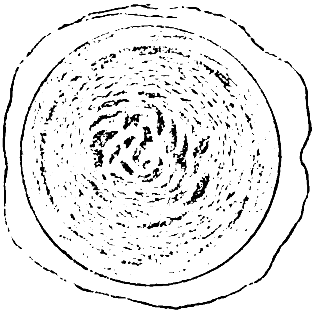


FIG. 6.

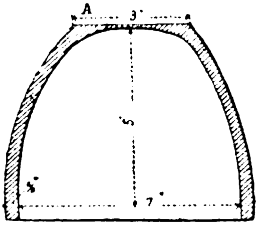


FIG. 7.

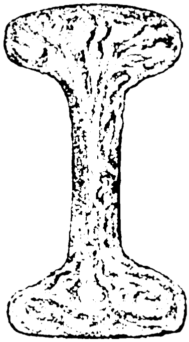
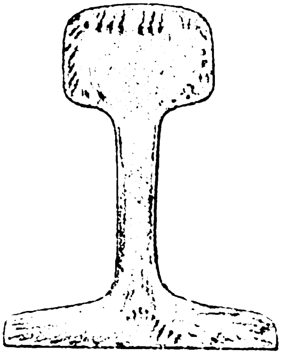


FIG. 8.



---

* 'Journ. Iron and Steel Inst.' vol. ii. 1885, p. 497.

in the facility of working, and safety belonging to a highly ductile material can hardly be exaggerated." I need not point to the extreme interest, in connection with my subject, of these remarks from so distinguished an authority.

The very ancient mechanical art of striking coin is wholly dependent on the flow of metals. There is a popular belief that the impression imparted to discs of metal during coinage, is merely the result of a permanent compression of the metal of which the disc is made. Striking a coin, however, presents a case of moulding a plastic metal, and of the true flow of metal, under pressure, into the sunk portions of the die. I once heard Mr. Ruskin say: "You stamp the figure of the cow on a pat of butter; why do you not impress the bee on honey?" Simply because honey is not plastic and is too viscous, and it flows at the ordinary atmospheric pressure. This medal, struck from a series of discs, will serve to show, when the discs are separated, the way the metal flows into the deepest portion of the die. If the alloy used be too hard, or if the thickness of the metal required to flow be insufficient, the impression will always be defective, no matter how many blows may be given by the press. In Mr. Browning's well-known poem, "The Ring and the Book," there is a subtle recognition of the viscous nature of very pure gold, which he characterises as "the oozing of the mine," while, with regard to the manufacture of the ring, he shows:

> "Since hammer needs must widen out the round
> And file emboss it fine with lily flowers,"

that this is only possible because gold behaves like the honey to which he compares it. I have chosen this reference to Browning because I happen to have a coin of "the great Twelfth Innocent," the Pope of the poem I have cited, and this coin has scars on its surface which prove that there was not quite enough metal to flow into the depths of the die.

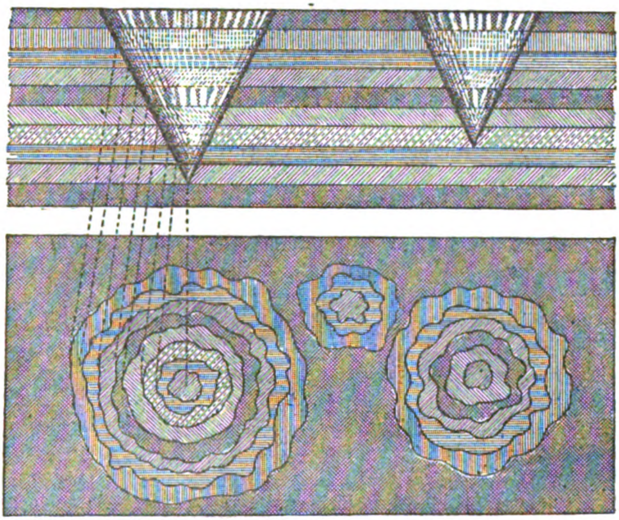
If one side of a coin be ground away so as to leave a flat surface, and if the disc be then struck between plain polished dies surrounded by a steel collar, so as to prevent the escape of the metal, the impression on the disc will be driven through the thickness of the metal, and will then appear on both sides. In industrial art the property of flow of metals is very important. The "spinning" of articles in pewter is a familiar instance, and one which I propose to illustrate.

[A disc of pewter was spun, on a lathe, into a vase, the shadow being projected on the screen during the operation.]

The production of complicated forms, like a jelly mould, from a single sheet of copper under the combined drawing and compressing action of the hammer, is a still more remarkable case.

The flow of metals is illustrated very curiously in one phase of Japanese art metal work, of which, however, it is now so difficult to obtain native examples, that I have been obliged to prepare, with the aid of skilful artificers in the Mint, the few specimens I have to submit to you. I allude to the metal work in banded alloys to which the Japanese give the name of Moku-me, or "wood-grain." In its preparation thin layers of copper, precious metals, and various alloys, are soldered in superposition like the leaves of a book; through these layers, as is shown in Fig. 9, holes are drilled

FIG. 9.



to varying depths in the thickness of the metal, or trenches are cut in it. The mass is then hammered flat until the hole or trenches disappear, and the result is contorted bands, of some complexity, and often possessing much beauty, especially when the colour of the metal is developed by suitable chemical treatment and polishing. A similar effect may be produced by beating up the metal from one side, and filing the other flat, as is shown in Fig 10. The structure depends on the flow of the respective metals of which the mass is composed, and behaviour of the components of the system, and suggests one of the most marked facts in experimental hydrodynamics, namely, the difference in the way in which water flows along contracting and expanding channels. The sinuous lines the metal assumes in the preparation of Moku-me, resemble the beautiful illustrations devised by Professor Osborne Reynolds, to show the flow of water.

We have hitherto only considered the flow of metals when submitted to compression, let us now examine the effect of traction. When a viscous metal, such as iron or soft steel, is submitted to stress by pulling its ends in opposite directions, it stretches uniformly throughout its length; there is in such a solid, a limit in the application of the stress up to which the metal, if released, will return to

FIG. 10.

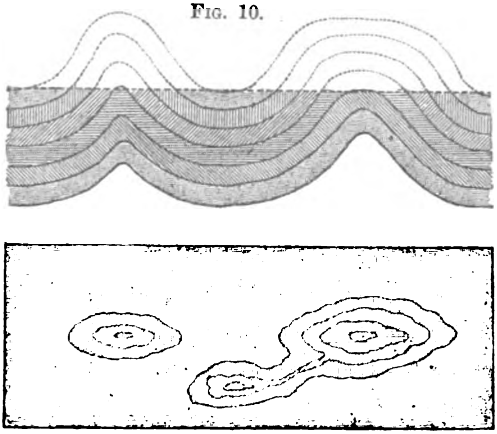
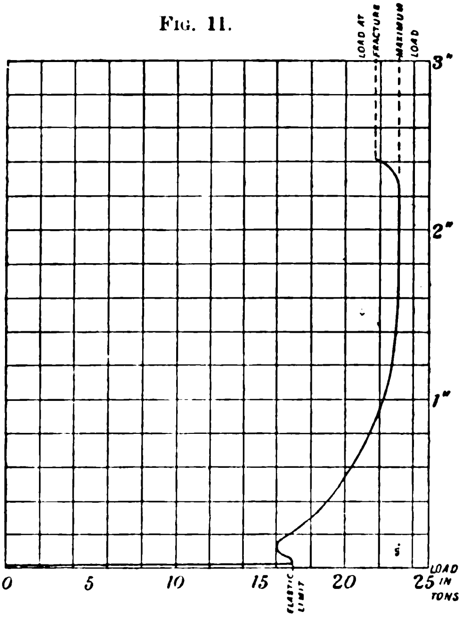
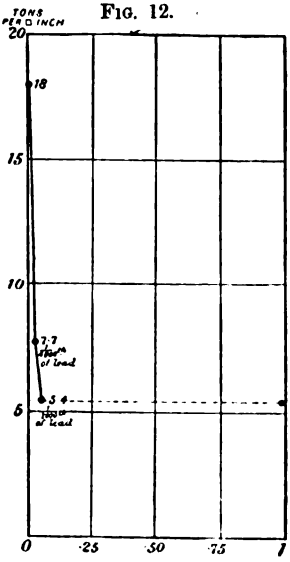


FIG. 11.



its normal length; this point is the limit of elasticity. Directly, however, this limit is reached, the metal begins to stretch, and at first it stretches in a very singular way, without an increase of load, as is shown by the curve, Fig. 11; when the limit of elasticity has been passed, the metal continues to stretch with increased load until it gives up resisting and breaks. The limit of elasticity of a solid body marks the moment at which the body begins to "flow" under the influence of the force to which it is submitted. There are many materials which do not stretch when their limit of elasticity is reached: in very hard steel, for instance, the breaking point and the limit of elasticity practically coincide. Further, it must be observed that every minute variation in composition is sufficient to change the property of a body, and to cause what was a viscous body to break close to the limit of elasticity. A most remarkable instance is presented by certain alloys of gold and copper. Standard gold, such as is employed for British gold coin, which contains 9167 parts of gold in 10,000 parts, breaks with a load of 18 tons to the square inch. Its limit of elasticity is reached at 1¾ tons per square inch, but it elongates 34 per cent. before it breaks. If this standard gold has only the $\frac{1}{2000}$th part of lead added to it, it becomes very brittle, and breaks, as is shown by the diagram, Fig. 12, with a stress of about 5½ tons to the square inch, instead of 18 tons borne by the original pure standard gold, and as it does not elongate sensibly it cannot be said to flow at all. A remarkable difference in the property of the alloy, standard gold, is therefore caused by the addition of only the $\frac{1}{2000}$th part of lead. In order to understand this it will be necessary to trace the analogy between fluids and solid metals still further, and to ascertain what takes place when metals, in a roughly granular state, are submitted to compression, under conditions in which the escape of the compressed metal is, as far as possible, restrained. I must, therefore, turn to what I believe to be the most important work relative to the molecular constitution of metals, which has been done for many years, namely, the researches of Professor Walthère Spring, of the University of Liége, whose labours have since 1878 been devoted to the study of the effect of compression on various bodies.* The particles of a metallic powder

Fig. 12.



Percentage of Lead.

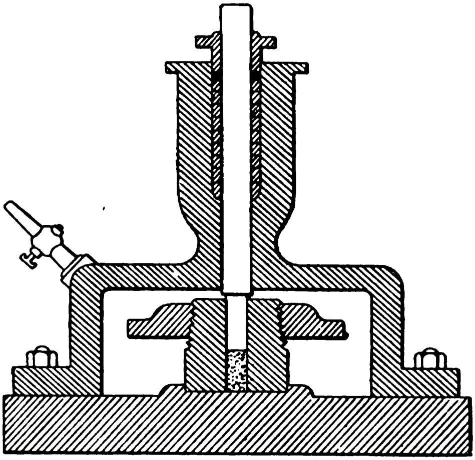
---

* 'Bull. de l'Acad. Royal de Belgique,' [2] t. xlv. No. 6, 1878. [2] t. xlix. No. 5, 1880. See also subsequent papers in the same publication, in the 'Bull. Soc. Chim.' Paris, and in the 'Deutsche Chemische Gesellschaft' (Bildung von Legirungen durch Druck), b. xv. p. 595.

left to itself at the ordinary atmospheric pressure, will not unite; by "augmenting the number of points of contact in a powder," the result may be very different. The powders of metals may weld into coherent blocks. Let us appeal to his own experiments. The section, Fig. 13, shows the general form of the compression apparatus employed by Spring.

Under a pressure of 2000 atmospheres on the piston, or 13 tons to the square inch, lead in the form of filings becomes compressed into a solid block in which it is impossible to detect the slightest vestige of the original grains, so that lead filings weld into a block identical with that obtained by fusion. Under a pressure of 5000 atmospheres the lead no longer resists the pressure, but flows as if it were liquid through the cracks of the apparatus, and the piston of the compressor descends to the base of the cylindrical hole driving the "flowing" lead before it. This result in the case of lead is hardly unexpected, for Bolley* showed in 1849, that lead prepared in a particular way, so as to be in a state of fine division, may be converted under a powerful press into a flexible plate, or may be made to receive a sharp impression. The more interesting results were

FIG. 13.



The metallic powder is placed under a short cylinder of steel in a cavity in a steel block divided vertically, held together by a collar, and placed in a chamber of gun-metal, which may be rendered vacuous. The pressure is applied to a cylindrical rod passing through the stuffing-box.

---

* 'Liebig u. Kopp. Jahresb.' 1849, p. 278, quoted by Dr. Percy, 'Metallurgy of Lead, 1870, p. 9.

obtained by Spring with crystalline metals. Bismuth is, as is well known, very brittle and crystalline, yet fine powder of bismuth unites under a pressure of 6000 atmospheres into a block very similar to that obtained by fusion, having a crystalline fracture. The density of compressed bismuth is 9·89, identical with that of metal which has been fused. The table shows the amount of pressure required to unite the powders of the respective metals:

	Tons per sq. inch.
Lead unites at	13
Tin ,,	19
Zinc ,,	38
Antimony unites at	38
Aluminium ,,	38
Bismuth ,,	38
Copper ,,	33
Lead flows at	33
Tin ,,	47

We will endeavour to repeat M. Spring's results in the case of bismuth. It is necessary that the powder be perfectly clean and dry, if it be then submitted *in vacuo* to a pressure of 6000 atmospheres, it will weld into a crystalline mass.

We know that combinations are produced when certain bodies in solution are submitted to each other's action. But do solids combine? Is the alchemical aphorism that bodies do not react unless they are in solution, true? Experiment proves that such solution is not necessary. I have here two anhydrous salts, iodide of potassium and corrosive sublimate, and they are at the same time dry; when they are mixed together in this mortar, they unite, as is shown by the vermilion colour which is produced.

My colleague, Professor Thorpe, called attention to the importance of this fact at the meeting of the British Association at York, 1881. But do solid metals combine in the sense, that is, in which chemical combination is possible between metals, when submitted to each other's action? We know that metals do combine if they be fluid, and the extraction of gold and silver from their ores by amalgamation is moreover easy. It occurred to M. Spring that if there be a true union between the particles of a metallic powder, when submitted to great pressure, it ought to be possible to build up alloys by compressing the powders of their constituent metals, and he urged that the formation of alloys by pressure would afford the most conclusive proof that there is a true union between the particles of metals in the cold, when they are brought into intimate contact. Experiment proved that this reasoning was correct, for by compressing, in a finely divided state, fifteen parts of bismuth, eight parts of lead, four parts of tin, and three parts of cadmium, an alloy is produced which fuses at 100° Cent. It is necessary, however, to compress the mixed powders twice, crushing or filing up the block obtained in the first compression, because the mechanical mixture of the constituent metals is not sufficiently intimate to enable a uniform alloy to be obtained by a single compression. The alloy we have thus produced fuses, you will observe, in boiling water, actually at 98° Cent., although the melting point of the most fusible of its constituents, the tin, is 228° Cent. I agree with Professor Spring in thinking that the formation of alloys by pressure affords the most complete proof which can be given of the accuracy of the views he has adduced. The formation of fusible-metal by compression leads me to deal with an objection which may, no doubt, have suggested itself to many of us. It may be urged that by compressing these metallic powders heat is evolved, and that this heat may be sufficient to produce incipient fusion in the metallic powders, or, at all events, may exert a material influence on the result obtained. This objection has been experimentally anticipated by Professor Spring. First, the compression is effected with extreme slowness, and therefore there can be no question as to the sudden evolution of heat, as would be the case 'if the powders were compressed by impact instead of by a slow squeeze; and to sum the matter up briefly, Spring calculates, taking an extreme case, that, if it be granted that all the work done in compressing the powders were actually translated into heat, it would only serve to heat a cylinder of iron 10 mm. in height and 8 mm. in diameter (the dimensions of cylinder produced in his apparatus), 40·64° Cent. In order that direct experimental evidence might not be wanting, Spring took the organic body phorone, a hard crystalline substance which melts at 28° Cent., and compressed it exactly as in the case of the metallic powders.* He took the precaution to place a shot of lead on the top of the powder before submitting it to compression: only imperfect union of the particles of phorone resulted. The conclusion of the experiment proved that the shot remained where it had been placed at the top of the column, and therefore the 28° necessary to melt the substance had not been evolved, for if it had the shot must have fallen through the fluid mass. I think, then, it is absolutely safe to conclude that, in the compression of bismuth, for instance, there can be no question of the evolution of the heat necessary for the fusion of the metal. There is, however, other evidence to which I may incidentally appeal. M. Spring has shown that by compressing powders together, chemical combination may be induced, and he has in this way produced arsenide and sulphide of zinc, sulphide of lead, and of bismuth, and arsenide of lead. These are not merely intimate mechanical mixtures. Take, for instance, the sulphide of magnesium produced by compression; it is soluble in hot water; treatment with dilute hydrochloric acid evolves sulphuretted hydrogen, which is not the case with mere mixtures of magnesium and sulphur. Further, Spring has shown that by pressure a body may be made to pass from one allotropic state to another. Plastic sulphur is, under a pressure of 6000 atmospheres, compelled to pass into the condition of octahedral sulphur, an allotropic state which possesses a greater density. And he points out that a solid metal (not powders of metals) may have cavities obliterated by pressure, but that matter cannot be permanently compressed by pressure, unless it can assume an allotropic state of greater density than the one it possesses at the moment of compression.* Now let me point to the evidence these experiments afford as to the relation between solid metals and fluids. Members of the Royal Institution will know that Faraday discovered, in 1850, that two fragments of ice pressed against each other will unite, tendency to their union being considerable when the fragments are near their melting point. We also know what splendid service the regelation of ice has afforded in the hands of Dr. Tyndall, in explaining the formation of glaciers. Ice owes its movement, not to viscosity, but to regelation, and the union of fragments of ice under compression is also due to regelation. The facts which have been appealed to, and the theories which have been formed, respecting the regelation of ice, are too well known to you to demand lengthy notice from me. I will only observe that bismuth, like ice, expands on solidifying, and although Faraday failed to establish the existence of a property similar to regelation in bismuth, an eminent engineer, Mr. Thomas Wrightson, to whom we owe a series of experiments on the fluid density of metals, has satisfied himself by experimental evidence, that regelation exists in bismuth. Now, in explaining Spring's results we are met by this difficulty; the union of the particles of the metals cannot, in all cases, be due to viscosity, because viscous bodies are always capable of being stretched, and we find the welding taking place between the compressed powders of bodies such as zinc and bismuth, which, when submitted to traction, will not stretch. Spring therefore asks, "Is it possible that regelation may have something to do with the union of the powders?" and he urges, "Is it safe to conclude that regelation is peculiar to water alone?" "It is difficult to believe," he adds, "that in the large number of substances which nature presents to us, but one exists possessing a property of which we can find only minute traces in other bodies. The sum of our chemical and physical knowledge is against such a belief, and therefore the phenomenon of regelation may be pronounced in ice without being absolutely wanting in other bodies. To ascertain whether this is so, it is necessary to submit various bodies to the conditions under which the phenomena can be produced." "What," he asks, "are these conditions?" and he answers, "The pressure supported by the body, a certain degree of temperature, and time."

---

* 'Bull. Soc. Chim.' Paris, 1884, t. xli. p. 488.

Helmholtz and Tyndall have shown that when the pressure is weak, the regelation of ice is effected slowly. Spring points out that nitrate of sodium and phosphate of sodium, in powder, left to themselves in bottles become coherent, and if the coherence in these and other chemical compounds is but weak, it is simply because the points of contact between the particles of powder are but few. If, on the other hand, metallic or other powder be submitted to strong compression, the spaces between the fragments become filled with the débris of the crushed particles, and a solid block is the result. Finally, it may be urged that this union of powders of solid metals under the influence of pressure, that is to say, the close approximation of the particles, can be compared to the liquefaction of gases by pressure. At the first view this comparison may appear rash or strained, but it is nothing if we accept the views of Clausius on the nature of gases and liquids. In a gas the molecules are free, but if by pressure at a suitable temperature the molecules are brought within the limits of their mutual attraction, the gas may be liquefied, and under suitable thermal conditions solidified. The mechanical pulverisation of a metal merely detaches groups of molecules from other groups, because the mechanical treatment is imperfect, but the analogy between the solid and a gas has, in a sense, been established; filing has coarsely gasified the mass, but pressure will solidify it as you have already seen.

---

* "Sur l'elasticité parfaite des corps solides chémiquement définis." 'Bull. Acad. Roy. Belgique,' [3] t. vi. 1883.

It is possible that in some of these metallic blocks, the particles are not actually united by the pressure, which may, nevertheless, develop the kind of "mutual attraction" contemplated by Sir W. Thomson as existing between two pieces of matter at distances of less than 10 micro-millimetres.

There are two other properties which solid metals possess, in common with certain fluids, to which I must briefly allude. The first is the power of dissolving gas, which metals in the solid colloid condition possess. I will not offer any experimental illustration on this point, because the work of Graham has been fully dealt with in this theatre by Dr. Odling, and I have, in a course of lectures recently delivered here, shown that just as solid palladium occludes hydrogen, so the alloy of rhodium and lead occludes oxide of nitrogen, which it gives up with explosive violence on heating *in vacuo*, suggesting an analogy with fluid nitro-glycerine. The last property I have to submit to you, is the power which certain solid metals possess of taking up fluids, sometimes with a rapidity which suggests the miscibility of ordinary fluid substances. In reference to this I have found an interesting paper published so long ago as 1713, by the Dutch chemist Homberg,* "On Substances which penetrate and which pass through Metals without melting them." He enumerates several substances which will pass through the pores of metals without disturbing the particles, and he points out that mercury penetrates metals without destroying them. Few of us are, I think, familiar with the rapidity with which mercury will pass through tin. Here is a bar, one inch wide and half inch thick; if a little mercury be rubbed lightly on it the mercury will in 30 seconds penetrate the mass, so that it breaks readily, although before the addition of the mercury, the bar would bend double without any sign of fracture.

---

* 'Mem. de l'Acad. Royale des Sciences,' 1713 [vol. for 1739, p. 306].

With regard to the vaporisation of solid metals, time will only permit me to remind you that Demarçay* has shown that *in vacuo* metals evaporate at much lower temperatures than they do at the ordinary atmospheric pressure, and he suggests that even metals of the platinum group will be found to be volatile at comparatively low temperatures. Merget † has shown that the solidification of mercury by extreme cold does not prevent the solid metal evaporating into the atmosphere surrounding it.

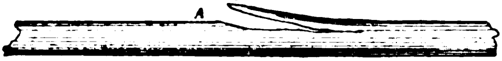
With regard to the remaining properties on my list, you will say, surely solids do not show any tendency to diffusion? I have shown ‡ that in the case of molten metals the interdiffusion may be extremely rapid, but, with regard to solid metals, some experiments conducted by Sir Frederick Abel prove that carbon can pass from a plate of richly carburised iron to one of iron free from carbon, against which it is tightly pressed. This passage of carbon takes place at the ordinary temperature, and it is difficult to explain the transference of matter without admitting the presence of some action closely allied to the diffusion of liquids.

Finally, can we offer any evidence of surface tension in solid metals? There is only one experiment to submit to you illustrating a point I am still investigating. Some months since Mr. F. W. Fletcher, manager of the works of Messrs. C. Ash and Sons, the well-known dealers in the precious metals, pointed out to me an interesting property of a hard-drawn rod or thick wire of 13-carat gold; the gold being alloyed with silver and copper in the following proportions:—

Gold	54·17
Copper	33·33
Silver	12·53
	100·00

If such a rod be touched with a solution of chloride of iron or certain other soluble chlorides, it will, in a short time, varying from a few seconds to some minutes, break away, as is shown in the diagram, Fig. 14, the fracture rapidly extending for a distance of some inches.

Fig. 14.



[The image of the rod was projected on the screen, and in a few seconds after the rod was touched with chloride of iron, it split close to the point of contact with the solution.]

---

* 'Comptes Rendus,' xcv. p. 183 [1882].

† 'Ann. de Chim. et de Phys.' [4], xxv. p. 121.

‡ 'British Association Report,' 1883, p. 402.

This result may be attended with the absorption of gas, but, in any case, it would appear that in the hard-drawn rod the surface is in a state of tension, which is released by the action of the chloride.

The facts we have considered afford additional evidence as to continuity in the properties of all kinds of matter, and serve as a connecting link with the work of the past, the importance of which is too often overlooked. I trust it will be evident that the analogy of solid metals to fluids has an important bearing on the labours of those who are striving to advance science, to develop art, or to promote the industrial well-being of this country.

[W. C. R.-A.]

WEEKLY EVENING MEETING,

Friday, April 2, 1886.

WILLIAM HUGGINS, Esq. D.C.L. LL.D. F.R.S. Vice-President,
in the Chair.

HOWARD GRUBB, Esq. F.R.S. F.R.A.S.

Telescopic Objectives and Mirrors: their Preparation and Testing.

IT would probably lend an additional interest to a technical subject such as I have to bring before you to-night, could I preface my description of the processes now employed in the construction of telescopic objectives by a short historical account of what has been attempted and achieved in the past, but time will not permit.

A very few words, however, on the history of glass manufacture are necessary.

As I pointed out last Saturday afternoon, Dollond's brilliant discovery, which afforded a means of achromatising objectives, rendered possible their construction of greater size and perfection than formerly, provided suitable material could be obtained. But the chromatic errors being removed, faults in the material hitherto masked by them, were detected, and it was not until after many years that Guinand, a lowly but gifted Swiss peasant, succeeded in producing glass discs of a considerable size and free from these defects.

The secrets of his process have been handed down in his own family to Mons. Feil, of Paris (one of his descendants), and also through M. Bontemps, who for a time was associated with Guinand's son, and afterwards accepted an invitation from Messrs. Chance Bros. & Co., of Birmingham, to assist them in an endeavour to improve that branch of their manufacture. Only these two houses, so far as I am aware, have succeeded in manufacturing optical discs of large size.

TESTING OF OPTICAL GLASS.

Let me here say a few words respecting the testing of optical glass; I mean of the material of the glass, quite apart from the optician's work in forming it into an objective. When received from the glass manufacturer it is sometimes in this state, roughly polished on both sides, and sometimes in this, in which as you see there are small windows only, facets as they are called, polished on the edges. In case of lenses for telescopic objectives, it is always well to have them roughly polished on the sides, to avoid the chance of having to throw away a lens after much trouble and labour has been spent on it.

There are only three distinct points to be looked to in the testing of optical glass; (1) General clearness and freedom from air-bubbles, specks, pieces of "dead metal," &c.; (2) Homogeneity; (3) Annealing.

The first is the least important, and needs no instructions for detection of defects, any one can see these. The second is much more important, and much more difficult to test.

The best test for homogeneity is one somewhat equivalent to Foucault's test for figure of concave mirrors.

The disc of glass should be either ground and polished to form a convex lens, or if that be not convenient, it should be placed in juxtaposition with a convex lens of similar or larger size, and whose excellence has been established by previous experience.

The lens or disc is then placed opposite some small brilliant light—a small gas flame generally suffices—and at such a distance that a conjugate focus is formed at other side and at a convenient distance. When the exact position of this focus is found, the eye is placed as nearly as possible so that the image of flame is formed on the pupil. On looking at it with the eye in this position, the whole lens should appear to be "full of light"; but at the slightest movement to one side the light will disappear and the lens appear quite dark. If the eye be now passed slowly backwards and forwards between the position showing light and darkness, any irregularity of density will be most easily seen.

Of course, like everything else, some experience is necessary.

The rationale of this is very obvious. When the eye is placed exactly at the focus of a perfect lens, the image formed on the pupil is very small, and the slightest movement of the eye will cause the light to appear and disappear. If the eye be not at the focus, the pencil of light will be larger, and consequently it will require a much greater movement of the eye to cause the light to disappear. Now if any portion of the lens be of a different density to the general mass, that portion will have a longer or a shorter focus; consequently while the light flashes off the general area of the lens quickly, it still remains on the defective portions.

By imitating this arrangement and substituting a camera for the eye and forming the focus of a small point of light on the stop of the lens, I have succeeded in photographing veins in glass, and sometimes have found this useful as a record.

The third point—that of proper annealing—is easily tested by the polariscope.

For small discs the usual plan is to hold them between the eye and a polarizing plane, such as a piece of glass blackened at back or a japanned surface, and look at them through the facets, using as an analyser a Nichols prism.

Larger sizes, which are polished on the surfaces, can be more easily examined. It is difficult to describe the appearances, but I will put a few discs into the lantern polariscope and endeavour to point out what amount of polarization may safely be permitted in discs of glass to be used for objectives.

The composition of metallic mirrors of the present day differs very little from that used by Sir Isaac Newton. Many and different alloys have been suggested, some including silver or nickel or arsenic; but there is little doubt that the best alloy, taking all things into account, is made with 4 atoms of copper and 1 of tin, which gives the following proportions by weight: copper 252, tin 117·8.

CALCULATION OF CURVES.

Having now obtained the proper material to work upon, the first thing necessary is to calculate the curves to give to the lenses, in order that the objective when finished may be of the required focus, and be properly corrected for the chromatic and spherical aberrations.

As this lecture is intended to deal principally with the technical details of the process, I do not intend to occupy your time for more than a few moments on this head, nor indeed is it at all necessary. In my lecture last Saturday I explained the principles of achromatism, and in many published works full and complete particulars are given as to the calculation of the curves—particulars which are sufficient, and more than sufficient, for the purpose.

Much has been discussed and written concerning the calculation of curves of objectives, and much care and thought has been bestowed by mathematicians on this subject, and so far as the actual constructors are concerned, a certain amount of veil is thrown over this part of the undertaking, as if there were a secret involved, and as if each had discovered some wonderful formulæ by which he was enabled to calculate the curves much more accurately than others.

I am sorry to have to dispel this illusion. Practically the case stands thus. The calculation of the curves which satisfy the conditions of achromatism and desired focus is a most simple one, and can be performed by any one having a very slight algebraical knowledge in a few minutes, provided the refractive indices and dispersive power of the glass be known. Both Messrs. Chance and Feil supply these data quite sufficiently accurately for small-size objectives. Speaking for myself, I am quite content to take the figures as given by these glass manufacturers for any discs up to 10 inches in diameter. If over that size, I grind and polish facets on the disc and measure the refractive and dispersive powers myself.

The calculations of the curves required to satisfy the conditions of spherical aberration are very troublesome, but fortunately these may be generally neglected.

Some years ago the Royal Society commissioned one of its members to draw up tables for the use of opticians, giving the curves required to satisfy the conditions of both corrections for all refractive and dispersive indices.

A considerable amount of labour was expended on this work, but in the end it was abandoned, for it was found that the calculation of these curves was founded on the supposition that all surfaces produced by the opticians were truly spherical; while the fact is, a truly spherical curve is the exception, not the rule. The slightest variation in the form or figure of the curve will produce an enormous variation in the correction for spherical aberration, and it was soon apparent that the final correction for spherical aberration must be left to the optician and not to the mathematician. *Object-glasses cannot be made on paper.* When I tell you that a sensible difference in correction for spherical aberration can be made by half an hour's polishing, corresponding probably to a difference in the first place of decimals in radii of the curves, you will see that it is practically not necessary to enter upon any calculation for spherical aberration. We know about what form gives an approximate correction; we adhere nearly to that, and the rest is done by figuring of the surface.

To illustrate what I mean. I would be quite willing to undertake to alter the curves of the crown or flint lens of any of my objectives by a very large quantity, increasing one and decreasing the other so as to still satisfy the conditions of achromatism, but introducing theoretically a large amount of positive or negative spherical aberration, and yet to make out of the altered lens an object-glass perfectly corrected for spherical aberration.

I am now speaking of ordinary sizes. For very large sizes it is usual to go more closely into the calculations; but I may remark that it is sometimes possible to make a better objective by deviating from the curves which give a true correction for spherical aberration and correcting that aberration by figuring, rather than by strictly adhering to the theoretical curves. So far then as any calculation is required, the ordinary formulæ given in the text-books may be considered amply sufficient.

Having now determined on the curves, we have to consider the various processes which the glass has to undergo from the time it is received in this form from the glass manufacturer to the time when it is turned out a finished objective.

The work divides itself into five distinct operations:—(1) Rough grinding; (2) Fine grinding; (3) Polishing; (4) Centering; (5) Figuring and testing.

1. The rough grinding or approximate shaping of the glass is a very simple process. The glass is cemented on a holder, and is held against a revolving tool supplied with sand and water, and of a shape which will tend to abrade whatever portions are necessary to be removed to produce the required curves. These diagrams will illustrate the various operations.

2. Fine grinding. The tools used for fine grinding are of this

form, and are made of either brass or cast iron. I prefer cast iron, except for very small sizes. They are grooved on the face, in the manner suggested by the late Mr. A. Ross, in order to allow the grinding material to properly distribute itself.

If two spherical surfaces be rubbed together they will, as may be supposed, tend to keep spherical; for the spherical is the only curve which is the same radius every part of its surface. If fine dry abrading powder be used between, the same result will be obtained; but if wet powder be used, the surface will no longer continue spherical, but will abrade away more on the centre and edge than in the zone between. It was to meet this difficulty that the late Mr. A. Ross devised the idea of the distributing grooves. The fine grinding process is the first of the series which calls for any skill on the part of the operator.

That the *modus operandi* of the grinding be the more easily understood, let me explain the principle of the process in a few words.

When two surfaces of unequal hardness are rubbed together with emery powder and water between the two, each little particle of the powder is at any given moment in either of these conditions:—(*a*) Imbedded into the softer surface; (*b*) Rolling between the two surfaces; (*c*) Sliding between the two surfaces.

Those particles which become imbedded in the softer material do no work in abrading it, and but little in abrading the harder. They generally consist of the finer particles, and are kept out of action by the coarser which are rolling or sliding between the surfaces. Further, those that are purely rolling do little or no work. The greater part of the work is performed by those particles which are facetted and which *slide* between the two surfaces.

As the grinder is always of a much softer material than the glass, there is much more friction between the grinder and these particles than between the glass and the same particles, and therefore they partially adhere to the grinder and are carried by it across the face of the glass. This being so, it is now easy to perceive what the best conditions for rapid grinding are. Not too little emery, for then there will not be enough of abrading particles; not too much, for then the particles will roll on each other and tend to crush and disintegrate each other instead of abrading the glass, but just sufficient to form a single layer of particles between the grinder and the glass surface.

In the grinding of the small lenses, I mean up to five or six inches diameter, it is usual to carry out the entire grinding processes by hand; above that size by machinery. Surfaces up to 12 or even 15 inches can be ground by hand; but the labour becomes severe, and for my part I am gradually reducing the size for which the hand grinding is used, as I find the machine work more constant in its effects.

The machinery used is the same as that employed for the polishing operation, and I shall describe it under that head further on.

In the fine grinding operation by hand, the glass is usually cemented on to a holder of this form, having (for smaller sizes) three pieces of cork, to which the lens is attached, this holder being screwed to a spud or nose on top of a post screwed to the floor. The operator having applied the proper quantity of moist emery powder between the grinder and the glass, proceeds to work the former over the latter in a set of peculiar strokes, the amplitude and character of which he varies according to circumstances, at the same time that he changes his position round the post every few seconds.

* * * * * *

Although, as I have shown, the harder material is abraded very much more than the softer, yet the softer (the grinder) suffers considerable abrasion as well as the glass, and the skill of the operator is shown by the facility with which he is able to bring the glass to the curve of the grinder without altering the curve or figure of the latter.

It is even possible for a skilled operator to take a lens of one curve and a grinder of say a deeper curve, and by manipulation to produce a pair of surfaces fitting together and of shallower curves than either.

MEASUREMENT OF THE CURVES.

In the early stages of grinding, gauges of the proper radius, cut out of sheet brass or sheet steel, are used for roughly testing the curves of the lenses; but when we get to the finer grinding process, it is necessary to have something much more accurate.

For this purpose a spherometer is used. It is made in various forms, generally with three legs terminating in three hardened steel points, which lie on the glass, and a central screw with fine thread, the point of which can be brought down to bear on the centre of the glass. In this way the versed sine of the curve for a chord equal to diameter of circle formed by these points is measured, and the radius of curve can be easily calculated from this.

I do not find the points satisfactory for regular work. They are apt to get injured or worn, and for ground surfaces are a little uncertain as one or other of the feet may find its way into a deep pit. This particular spherometer has three feet, of about half an inch long, which are hardened steel knife-edges forming three portions of an entire circle. In using this it is laid on the surface to be measured, and the screw with micrometer head is turned till the point is felt to touch the surface of glass. This scale and head can then be read off. The screw in this instrument has fifty threads to the inch, and the head is divided into 100 parts, so that each division is equal to $\frac{1}{5000}$ of an inch. With a little practice it is easy to get determinate measures to $\frac{1}{10}$ of this, or $\frac{1}{50000}$ of an inch, and by adopting special precautions even more delicate measures can be taken, as far probably as $\frac{1}{100000}$ or $\frac{1}{150000}$ of an inch, which I have found to be practically the limit of accuracy of mechanical contact.

To give an idea of the delicacy of the instrument, I bring the screw firstly into contact with the glass. Now the screw is in good contact; but there is so much weight still on the three feet, that if I attempt to turn it round, the friction on the feet oppose me, and it will not stir except I apply such force as will cause the whole instrument to slide bodily on the glass. Now, however, I raise the whole instrument, taking care that my hands touch none of the metal-work, and that the screw be not disturbed. I lay my hands for a moment on part of the glass where centre screw stood, and thus raise its temperature slightly, and on laying the spherometer back in the same place, you now see that it spins on the centre screw, showing how easily it detects what to it is a large lump, caused by expansion of the glass from the momentary contact of my hand.

FLEXURE.

One of the greatest difficulties to be contended with in the polishing of large lenses is that of flexure during the process.

It may appear strange that in discs of glass of such considerable thickness as are used for objectives, any such difficulty should occur; but a simple experiment will demonstrate the ease with which such pieces of glass can be bent, even under such slight strain as their own weight.

We again take our spherometer and set it upon a polished surface of a disc of glass of about 7½ inches diameter and ¾ inch thick. I set the micrometer head as in the former experiment to bear on the glass, but not sufficiently tight to allow the instrument to spin round. This has now been done while the glass as you see is supported on three blocks near its periphery. I now place one block under the centre of disc and remove the others thus, and you see the instrument now spins round on centre screw.

It is thus evident that not only is this strong plate of glass bending under its own weight, but it is bending a quantity easily measurable by this instrument, which as I shall presently show is quite too coarse to measure such quantities as we have to deal with in figuring objectives.

After this experiment, no surprise will be felt when I say that it is necessary to take very special precautions in the supporting of discs during the process of polishing, to prevent danger of flexure; of course, if the discs are polished while in a state of flexure the resulting surface will not be true when the cause of flexure is removed.

For small-sized lenses no very special precautions are necessary, but for all sizes over 4 inches in diameter I use the equilibrated levers devised by my father, and utilised for the first time on a large scale in supporting the 6-foot mirror of Lord Rosse's telescope. These have been elsewhere frequently described, but I have a small set here as an example.

I have also sometimes polished lenses while floating on mercury.

This gives a very beautiful support, but it is not so convenient, as it is difficult to keep the disc sufficiently steady while the polishing operation is in progress, without introducing other chances of strain.

So far I have spoken of strain or flexure during the process of working the surface; but even if the surface be finished absolutely perfectly, it is evident from the experiment I showed you that very large lenses when placed in their cells must suffer considerable flexure from their own weight alone, as they cannot then be supported anywhere except round the edge.

To meet this, I proposed many years ago to have the means of hermetically sealing the tube, and introducing air at slight pressure to form an elastic support for the objective, the pressure to be regulated by an automatic arrangement according to the altitude. My attention was directed to this matter very pointedly a few years ago from being obliged to use for the Vienna 27-inch objective a crown lens which was according to ordinary rules much too thin.

I had waited some years for this disc, and none thicker could be obtained at the time. This disc was very pure and homogeneous, but so thin that if offered to me in the first instance I would certainly have rejected it. Great care was taken to avoid flexure in the working, but to my great surprise I found no difficulty whatever with it in this respect. This led me to investigate the matter, with the following curious results.

If we call f the flexure for any given thickness t, and f' the flexure for any other thickness t', then $\frac{f}{f'} = \frac{t^2}{t'^2}$ for any given load or weight approximately. But as the weight increases directly as the thickness, the flexure of the discs due to their own weight, which is what we want to know, may be expressed as $\frac{f}{f'} = \frac{t}{t'}$.

Let us now consider the effect of this flexure on the image. In any lens bent by its own weight, whatever part of its surface is made more or less convex or concave by the bending, has a corresponding part bent in the opposite direction on the other surface, which tends to correct the error produced by the first surface. This is one reason why reflectors which have not this second correcting surface are so much more liable to show strain than refractors. If the lens were infinitely thin, moderate flexure would have no effect on the image. The effect increases directly as the thickness. If then the flexure, as I have shown, decreases directly as the thickness, and the effect of that flexure increases directly as the thickness, it is clear that the effect of flexure of any lens due to its own weight will be the same for all thicknesses; in other words, no advantage is gained by additional thickness.

This has reference, of course, only to flexure of the lens in its cell after it has been duly perfected, and has nothing to do with the extra difficulty of supporting a thin lens during the grinding and polishing processes.

POLISHING.

The polishing process can be, and is often, conducted precisely in the same manner as the grinding, except that the abrading powders used (oxide of iron, rouge, an oxide of tin putty-powder) is of a finer and softer description, and the surface of the polishing tool is made of a softer material than the metallic grinder.

Very nearly all my polishing is done on the machine I shall presently describe; but before doing so, I will, with your permission, say a few words on the general principles of the polishing process. Various substances are used for the face of the polisher—fine cloth, satin or paper, and pitch. Pitch possesses two important qualities which render it peculiarly suitable for this work, and it is a curious fact that we owe its application for this purpose to the extraordinary perspicuity of Sir Isaac Newton, who we may fairly say was the first to produce an optically perfect surface, and that that material is not only still used for the purpose, but is as far as I know the only substance which possesses the peculiar qualifications necessary to fulfil the required conditions. With skill and care, moderately good surfaces can be obtained from cloth polishers; but it is easy to see why they can never be perfect. There is a certain amount of elasticity in cloth and in its "nap," and there is consequently a tendency to round off the surfaces of the pits left by the grinding powder, and to polish the bottom or floor of these pits at the same time as the upper surface. It is easy to show mathematically that any process which abrades the floors of the pits at the same time as general surfaces even in a very much less degree, can never produce more than an approximation to a perfect surface, and practice agrees with the theory. Paper is said to be much used by the French opticians. I can say nothing about it. I have tried it and failed to produce a perfect surface with it, nor indeed should I expect it. It is of course open to the same objection as cloth. Pitch possesses, as I said, two most important qualities which render it suitable for the work; the first, in its almost perfect inelasticity; the second, a curious quality of subsidence, which we utilise in the process.

If we watch with a microscope, or even a magnifier, the character of two surfaces during the process of polishing, the one with cloth, and the other with pitch, the difference is very striking. With the cloth polisher, the polish appears much quicker, and it would at first sight appear as if the same polishing powder abraded more quickly on the cloth than on the pitch polisher, but I do not believe that such is the case, for if we look at the surface with a magnifier we shall find that while all the surface has assumed a polished appearance, the surface itself has retained some of the form of the original pitted character with the edges rounded off; while in the pitch half-polished surfaces, the floors of the pits are as grey as ever, and the edges are sharp and decisive. In pitch polishing, too, a decided amount of polish appears very quickly, and then for many hours there appears to be little or no further effect. Suddenly, however, the remaining greyness disappears, and the surface is polished. The reason of this is very obvious. The polisher being very inelastic, polishes first only the tops of the hills, and has to abrade away all the material of which these hills are composed before it reaches the valleys or floors of the pits. When it does reach them, the proper polish quickly appears. The second quality of pitch, that of subsidence, is also most valuable.

Pitch can be rendered very hard by continued boiling. By pitch I mean the natural bituminous deposit which comes to us from Archangel, not gas-tar pitch. It can be made so hard that it is impossible to make any impression on it with the finger-nail without splitting it into pieces; and yet even in this hard condition if laid on an uneven surface it will in a few days, weeks, or months subside and take the form of whatever it is resting upon. The cohesion of its particles is not sufficient to enable it to retain its form under the action of gravity; and as this condition is that which science tells us marks the difference between solids and liquids, we must, paradoxical though it may appear, class even the hardest pitch among liquid instead of solid substances.

Now how do we utilise this peculiar quality.

The polishing tool is made by overlaying a metal or wooden disc formed to nearly the required curves by a set of squares of pitch, and while these are still warm pressing them against the glass, the form of which they immediately take.

In the grinding process, I showed you that the regulation of the abrasion was managed partly by the character of the stroke given, and partly by the local touches given to the tool by the stoning process.

In polishing we still retain the same facilities for modifying the stroke, and the same rules I gave apply generally to the polishing process as well as the grinding; but we have not got any process equivalent to that of the local stoning, and even if we had it would be useless, for this very quality of subsidence of the pitch would in a few minutes cause any part of its surface which had been reduced to come into good contact again; we must therefore look for some other means for producing more or less abrasion whenever we require it. This we effect by modifying the size of the squares of pitch in the various zones. Practically it is done in this way by a knife and mallet. Whenever the squares are reduced, the abrasion will be less.

This is a well-known method of regulation; but the rationale is, I think, not generally understood. It is generally explained that there is less abrasion because there is less abrading surface. I do not think this is the true, or at least the entire, explanation. In order to understand the action, you must conceive the pitch to be constantly in a state of subsidence, the amount of that subsidence depending of course on the pressure placed upon it. Now if we reduce the size of the squares in any zone while retaining the same distance from centre to centre of squares, we increase at first the pressure per unit of area on the pitch squares in that zone, and consequently the subsidence will be greater, and the action will not be so tight or severe on that zone.

I know of no substance but pitch and a few of the resins which possess this peculiar quality except perhaps ice, and it is curious to think that the same quality which in ice allows of that gradual creeping and subsidence and consequent formation of glaciers with their characteristic moraines, &c., will in pitch help us to produce accurate optical surfaces.

POLISHING MACHINES.

The two best-known polishing machines are those of the late Earl of Rosse and the late Mr. Lassell, the general forms of which are shown in these diagrams. Time will not permit me to enter into a minute description, of their working, nor is it necessary, as both have been often described.

A few words, however, as to the different character of the strokes given by these machines may be interesting. The stroke of Lord Rosse's machine may be imitated in hand-work by the operator traversing the polisher or mirror in a series of nearly straight strokes, of about one-third the diameter of the glass, to and from himself, at the same time that he keeps walking slowly round the post, and instead of allowing the centre of polisher to pass directly over the centre of mirror, each stroke that he gives he slides a little (about one-tenth diameter) to one side and then a little to the other.

Mr. Lassell's stroke may be imitated by causing the polisher to describe a series of nearly circular strokes a little out of the centre, walking round the post at the same time; thus the centre of polisher will describe a series of epicycloidal or hypocycloidal curves on the speculum.

Many years ago my father devised a machine, figured and described in Nichols' 'Physical Science,' by which either of these motions could be obtained. He appeared to have got better results with Mr. Lassell's strokes, for he afterwards devised a machine which gave the same character of stroke, but over which the operator had greater control, and this machine has been used for many years with great success. Like all machines, however, which give a series of strokes constantly recurring of the same amplitude, it is apt to polish in rings. It is impossible to obtain absolute homogeneity in the pitch patches, and if any one square be a shade harder than the general number, and that square ends its journey at each stroke at the same distance from the centre of speculum or glass, there will almost surely be a change of curvature in that zone. To avoid this I have made a slight modification in the machine, which has increased its efficiency to a great extent. I will now place in the lantern a model of this machine, and first draw you a few curves with the machine in its old state, and afterwards in its improved state.

In order to convey some idea of the relative quantities of material removed by the various processes, I have placed upon the walls a diagram which will illustrate this point in two distinct ways.

The diagram itself represents a section of a lens of about 8 inches aperture and 1 inch thick, magnified 100 times, and shows the relative thickness of material abraded by the four processes.

The quantity removed by the rough grinding process is represented on this diagram by a band 25 inches wide, the fine grinding by one $\frac{8}{10}$ inch wide, the polishing by a line $\frac{1}{50}$ inch wide, while the quantity removed by the figuring process cannot be shown even on this scale as it would be represented by a line only $\frac{1}{10000}$ inch thick.

I have also marked on this diagram the approximate cost of abrasion of a gramme of material by each of the four processes, viz :—

	£	*s.*	*d.*	
Rough grinding, about	0	0	1	per gramme.
Fine grinding, „	0	0	7½	„
Polishing, „	0	10	0	„
Figuring, „	48	0	0	„

FIGURING AND TESTING.

By the figuring process, I mean the process of correcting local errors in the surfaces, and the bringing of the surfaces to that form, whatever it may be, which will cause the rays falling on any part to be refracted in the right direction. When an objective has undergone all the processes I have described, and many more which are not so important, and with which I have not had time to deal, and when the objective is centered and placed in its cell, it is, to look at, as perfect as it will ever be, but to look through and use as an objective it may be useless. The fact is that when an objective has gone through all the processes described, and is in appearance a finished instrument, I look upon it as about one-fourth finished. Three-fourths of the work has probably to be done yet. True, sometimes this is by no means the case, and I have had instances of objectives which were perfect on the first trial; but this is, I am sorry to say, the exception and not the rule.

This part of the process naturally divides itself into two distinct heads :—

1. The detection and localisation of faults—what may, in fact, be termed the diagnosis of the objective.

2. The altering of the figures of the different surfaces to cure these faults. This may be called the remedial part.

It may be well here to try to convey some idea of the quantities we have to deal with, otherwise I may be misunderstood in talking of great and small errors.

I have before mentioned that it is possible to measure with the spherometer quantities not exceeding $\frac{1}{50000}$ of an inch, or with special precaution much less even than that; but useful as this instrument is for giving us information as to the general curves of the surface, it is utterly useless in the figuring process, that is, an error which would be beyond the power of the spherometer to detect, would make all the difference between a good and a bad objective.

Take actual numbers and this will be evident. Take the case of a 27-inch objective of 34 feet focus; say there is an error in centre of one surface of about 6 inches diameter, which causes the focus of that part to be $\frac{1}{10}$ of an inch shorter than the rest.

For simplicity sake, say that its surface is generally flat; the centre 6 inches of the surface therefore, instead of being flat, must be convex and of over 1,000,000 inches radius, about 17 miles. The versed sine of this curve, as measured by spherometer, would be only about $\frac{1}{250000}$, 4 millionths of an inch, a quantity mechanically unmeasurable, in my opinion.

If that error was spread over 3 inches only instead of 6 inches, the versed sine would only be about $\frac{1}{1000000}$. Probably the effect on the image of this 3-inch portion of $\frac{1}{10}$ inch shorter focus would not be appreciable on account of the slight vergency of the rays, but a similar error near edge of objective certainly would be appreciable. Until therefore some means be devised of measuring with certainty quantities of 1 millionth of an inch and less, it is useless to hope for any help from mechanical measurement in this part of the process.

If then no known mechanical arrangement be delicate enough to measure these quantities, how, it may be asked, are these errors detected?

The answer to this is, that certain optical arrangements enable us to carry our investigations far beyond the limits of mechanical accuracy. Trials of the objective or mirror as a telescope are really the crucial test, but there are various devices by which defects are detected and localised.

The best object to employ is generally a star of the third or fourth magnitude, when such is available, but as it frequently occurs that no such object is visible, recourse is had to artificial objects. The minute image of the sun reflected from little polished balls of speculum metal, or even a thermometer bulb is a very good object; polished balls of black glass are also used with good effect; but as the sun also is of somewhat fickle disposition in this country, we have frequently to have recourse to artificial light. Small electric lamps, such as this, with their light condensed, and thrown on a polished ball are very useful. In fact, I am never without one of them in working order.

For the detection and localisation of errors it is very useful to be provided with sets of diaphrams which leave exposed various zones of the surface, the foci of which can then be separately measured, but a really experienced eye does not need them.

For concave surfaces, Foucault's test is useful. I shall not trespass on your time to explain this in detail, as it is described very fully in many works, in none better than in Dr. Draper's account of the working of his own reflecting telescope. This diagram will give an idea of the principle of the system, which is really the same as what I have described as useful for detecting want of homogeneity in the substance of the glass.

This system is extremely useful for concave spherical surfaces, but is not available for convex surfaces, and only partially available for concave parabolic surfaces.

The really crucial test is, as I said before, the performance of the objective when used as a telescope; and the appearance of the image not only at the focus, but on each side of it, conveys to the practised eye all the information required for the detection of the errors.

If an objective have but one single fault, its detection is easy; but it generally happens that there are many faults superposed, so to speak. There may be faults of achromatism, and faults of figure in one or all the surfaces; faults of adjustment, and perhaps want of symmetry from some strain or flexure; and the skill of the artist is often severely taxed to distinguish one fault from the rest and localise it properly, particularly if, as is generally the case, there be also disturbances in the atmosphere itself, which mask the faults in the objective, and permit of their detection only by long and weary watching for favourable moments of observation.

It would be impossible in one or a dozen of such lectures as this to enumerate all the various devices that are practised for the localisation of errors, but a few may be mentioned, some of which have never before been made public.

For detection of faults of symmetry, it is usual to revolve one lens on another, and watch the image. In this way it can generally be ascertained whether it is in the flint or crown lens.

With some kinds of glass the curves necessary for satisfying the conditions of achromatism and spherical aberration are such that the crown becomes an equi-convex and the flint a nearly plano-concave of same radius on inside curve as either side of the crown. This form is a most convenient one for the localisation of surface errors in this manner.

The lenses are first placed in juxtaposition and tested. Certain faults of figure are detected. Now calling the surfaces A B C D in the order in which the rays pass through them, place them again together with Canada balsam or castor-oil between the surfaces B and C, forming what is called a cemented objective. If the fault be in either A or D surface, no improvement is seen; if in B or C the fault will be much reduced or modified. Now reverse the crown lens cementing surfaces A and C together. If same fault still shows, it must be in either B or D. If it does not show, it will be in either A or C. From these two experiments the fault can be localised.

It often happens that a slight error is suspected, but its amount is o slight that it appears problematical whether an alteration would eally improve matters or not. Or, the observer may not be able to nake up his mind as to the exact position of the zone he suspects o be too high or too low, and he fears to go to work and perhaps do arm to an objective on which he has spent months of labour, and vhich is almost perfect. In many such cases I have wished for some neans by which I could temporarily alter the surface and see it so ltered before actually proceeding to abrade and perhaps spoil it.

During my trials with the great objective of Vienna, I thought of very simple expedient, which effects this without any chance at all f injuring the surface. If I suspect a certain zone of an objective s too low, and that the surface might be improved by lowering he rest of it, I simply pass my hand, which is always warmer than he glass, some six or eight times round that particular zone. The ffect of this in raising the surface is immediately apparent, and is enerally too much at first, but the observer at eye end can then uietly watch the image as the effect goes off, and very often most seful information is thus obtained. The reverse operation, that of owering any required part of the surface, is equally simple. I take bottle of sulphuric ether and a camel's-hair brush, and pass the rush two or three times round the part to be lowered, blowing on it lightly at the same time; the effect is immediately perceived, and can lways be overdone if required.

So far then for the diagnosis. Now for the remedy. When the ault has been localised, the lens is again put upon the machine and he polisher applied as before, the stroke of the machine and the ize of the pitch patches being so arranged as to produce, or tend to roduce, a slightly greater action on those parts that have been found be too high (as before described while treating of the polishing rocesses).

The regulation of the stroke, excentricity, speed, and general action f the machine, as well as the size and proportion of the pitch quares, and the duration of the period during which the action is to e continued, are all matters the correct determination of which epends upon the skill and experience of the operator, and conerning which it would be impossible to formulate any very definite ules. All thanks are due to the late Lord Rosse and Mr. Lassell, nd also to Dr. de la Rue, for having published all particulars of the rocess which they found capable of communication; but it is a otable fact that, as far as it is possible to ascertain, every one who as succeeded in this line has done so, not by following written r communicated instructions, but by striking out a new line for imself; and I think I am correct in saying that there is hardly be found any case of a person attaining notable success in the art f figuring optical surfaces by rigidly following directions or instrucons given or bequeathed by others.

There is one process of figuring which is said to be used with uccess among Continental workers. I refer to the method called the process of local touch. In this process those parts, and those parts only, which are found to be high are acted upon by a small polisher.

This action is of course much more severe; and if only it were possible to know exactly what was required, it ought to be much quicker; but I have found it a very dangerous process. I have sometimes succeeded in removing a large lump or ring in this way (by large I mean 3 or 4 millionths of an inch), but I have also and much oftener succeeded in spoiling a surface by its use. I look upon the method of local touch as useful in removing gross quantities, but for the final perfecting of the surface I would not think of employing it.

In small-sized objectives the remedial process is the most troublesome, but in large-sized objectives the diagnosis becomes much the more difficult, partly on account of the rare occurrence of a sufficiently steady atmosphere. In working at the Vienna objective it often happened when the figure was nearly perfect that it was dangerous to carry on the polishing process for more than ten minutes between each trial, and we had then sometimes a week to wait before the atmosphere was steady enough to allow of an observation sufficiently critical to determine whether that ten minutes' working had done harm or good. It must not be supposed either that the process is one in which improvement follows improvement step by step till all is finished. On the contrary, sometimes everything goes well for two or three weeks, and then from some unknown cause, a hard patch of pitch perhaps, or sudden change of temperature, everything goes wrong. At each step, instead of improvement there is disimprovement, and in a few days the work of weeks or months perhaps is all undone. Truly any one who attempts to figure an objective requires to have the gift of patience highly developed.

In view of the extraordinary difficulty in the diagnostic part of the process with large objectives, it is my intention to make provision which I hope may reduce the trouble in the working of the new 28-inch objective for the Royal Observatory, Greenwich.

Two of the greatest difficulties we have to contend with are: 1stly, the want of homogeneity in the atmosphere, through which we have to look in trials of the objective, due to varying hygrometric and thermometric states of various portions; and 2ndly, sudden changes of temperature in the polishing room. The polisher must always be made of a hardness corresponding to the existing temperature. It takes about a day to form a polisher of large size, and if before the next day the temperature changes 10° or 15°, as it often does, that polisher is useless, and a new one has to be made, and perhaps before it is completed another change of temperature occurs. To grapple with these two difficulties I propose to have the polishing chamber underground, and leading from it a long tunnel formed of highly glazed sewer-pipes about 350 feet long, at the end of which is placed an artificial star illuminated by electric light; on the other side of the polishing chamber is a shorter tunnel, forming the tube of the telescope, terminating in a small chamber for eye-pieces and observer. About half-way in the long tunnel there will be a branch pipe connected to the air shaft of the fan, which is used regularly for blowing the blacksmith's fire, and through this when desired a current of air can be sent to "wash it out" and mix up all currents of varying temperature and density. It may be found necessary even to keep this going during observations.

By this arrangement I hope to be able to have trials whenever required, instead of having to wait hours and days for a favourable moment.

FIGURING OF PLANE MIRRORS.

There is a general idea that the working of a plane mirror or one of very long radius is a more difficult operation than those of more ordinary radii. This is not exactly the case. There is no greater difficulty in figuring a low curve than a deep one, but the difficulty in the case of absolutely plane mirrors consists simply in the fact that in their figuring there is one additional condition to be fulfilled, viz. that the general radius of curvature must be made accurate within very narrow limits. In figuring a plane mirror to use, for instance, in front of even a small objective, say 4-inch aperture, an error in radius which would cause a difference of focus of $\frac{1}{100}$ of an inch would seriously injure the performance. This would be about equivalent to saying that the radius of curvature of the mirror was about 8 miles, the versed sine of which with the 6-inch spherometer would be about $\frac{1}{50000}$ of an inch. Now what I mean to convey is this; that it would be just as difficult to figure a convex or concave lens of moderate curvature as a flat lens of the same size if it were necessary to keep the radius accurate to that same limit, i.e. one-tenth of a division of this spherometer.

LICK OBSERVATORY.

For the final testing of large objectives or mirrors, it is necessary to have them properly mounted, and in such a manner that they can be directed conveniently on any celestial object, and kept so directed by clockwork to enable the observer to devote his whole attention to the testing. I had not intended touching at all on the subject of the mounting of telescopes, but I have been asked to call your attention to this model of a proposed observatory for Mount Hamilton, California, as it embraces some novel features, but I shall do so in only a very few words.

Most here are probably aware that a monster observatory is in course of erection in California, a large sum of money having been left for the purpose by a Mr. Lick. The observatory is already partly complete, and contains some excellent instruments of moderate size, the work already done with which warrants us to hope that the great 36-inch refractive about to be erected will be placed under more favourable conditions for work than any other large telescope in the world.

The 36-inch objective is at present in process of construction by the Messrs. Clark of America, but the mounting has not yet been contracted for.

Some years since, in a paper published in the Transactions of the Royal Dublin Society, I shadowed forth a principle which I considered should be adopted in great telescopes of the future. The trustees of the Lick observatory having invited me to design an instrument for the 36-inch objective, I have put into practical form what I had before given but general principles of, and the design which this model illustrates is the result.

Whether this design will ever be carried out or not I cannot tell, but even as a proposal I trust it may be interesting enough to excuse my introducing it (somewhat irrelevantly perhaps) to your notice to-night.

The design includes the equatorial itself, with its observatory, dome, and provision for enabling the observer to reach the eye end conveniently.

The conditions I laid down for myself in designing this observatory were that it would be possible for the observer single-handed to enter the equatorial room at any time, and that, without using more physical exertion than is necessary for working the smallest-sized telescope, or even a table microscope, he should be able to open the 70-foot dome, turn it round backwards and forwards, point the equatorial to any part of the heavens, revolving it in right ascension and declination to any extent, and finally (the most difficult of all) to bring his own person into a convenient position for observing. I say this last is the most difficult of all, for I think any who have worked with larger instruments will allow that there is generally far more trouble in moving the observatory chair (so called) and placing it in proper position than in pointing the instrument itself. In this instrument the "chair" would require to be 25 feet high, and with its movable platform, ladder, balance-weight, &c., would weigh probably some tons. Even if very perfect arrangements were made for the working of this chair, the mere fact that the observer while attempting to make the most delicate observations is perched upon a small and very unprotected platform 25 feet above the floor and in perfect darkness, tends to reduce his value as an observer to an extent only to be appreciated by those who have tried it.

No matter how enthusiastic a man may be at his work, I would not put a high value on his determinations if made while in a position which calls for constant anxiety for his own personal safety. I would go even further still, and say that even personal comforts or discomforts have much to do with the value of observations.

I propose, therefore, that all the various motions should be effected by water power. There are water engines of various forms now made, some of which have no dead point, and having little vis inertia, are easily stopped and started, and are consequently well adapted for this work.

I propose to use four of them: one for the right ascension motion of the instrument, and one for the declination; one for revolving the dome, and one for raising and lowering the observer himself; but instead of having anything of the nature of a 25-foot chair or scaffold, I propose to make the 70-foot floor of the observatory movable. It is balanced by counterpoise weights, and raised and lowered at will by the observer. Then the observer can without any effort raise and lower the whole floor, carrying himself and twenty people if desired, to whatever height is most convenient for observation; and wherever he is observing, he is conscious that he has a 70-foot floor to walk about on, which even in perfect darkness he can do in safety.

The valves and reversing gear of the water engines are actuated by a piece of mechanism, the motive power of which may be a heavy weight raised into position some time during the previous day by man or water power. By means of a simple electrical contrivance, this piece of machinery itself is under the complete control of the observer, in whatever part of the room he may be, and he carries with him a commutator of a compact and convenient form, with eight keys in four pairs, each pair giving forward and backward movements respectively to

A. Telescope movement in right ascension;
B. Telescope movement in declination;
C. Revolution of dome;
D. Raising of floor.

The remaining operation, opening of shutter, is easily effected without any additional complication.

It is only necessary to anchor the shutter (which moves back horizontally) to a hook in the wall and move the dome in the opposite direction by motion C; the shutter must of course be opened by this motion.

It is very possible that there may be some here who have found what I have had to say on the subject of the figuring of objectives very unsatisfactory. They may have expected, naturally enough, that instead of treating of generalities to such a large extent as I have done, I should have given precise directions, by the following of which rigidly any person could make a telescopic objective.

To those, however, who have followed me in my remarks, the answer to this will probably have already suggested itself. It is the same answer which I give to those who visit my works and ask what the secrets of the process are, or if I am not afraid that visitors will pick up my secrets. All the various processes which I have described up to that of the figuring are I consider purely mechanical processes, the various details of which can be communicated or described as any mechanical process can be; but in the last final and most important process of all there is something more than this. A person might spend a year or two in optical works where large objectives are made, and might watch narrowly every action that was taken, see every part of the process, take notes, and so forth, and yet he could no more expect to figure an objective himself, than a person could expect to be able to paint a picture because he had been sitting in an artist's studio for the same time watching him at his work. Experience and experience only can teach any one the art, and even then it is only some persons who seem to possess the power of acquiring it.

A well-known and experienced amateur in this work declared his conviction that no one could learn the process under nine years' hard work, and I am inclined to think his estimate was not an exaggerated one.

True, it may be said that large objectives can be and are generally turned out by machinery, but what kind of an objective would any machine turn out if left to guide itself, or left to inexperienced hands?

At the risk of being accused of working by what is generally called the rule of thumb, I confess that conditions often arise, to meet which I seem to know intuitively what ought to be done, what crank to lengthen, what tempering is required of the pitch squares; and yet if I were asked I should find it very hard to give a reason for my so doing which would even satisfy myself.

I may safely say that I have never finished any objective over 10 inches diameter, in the working of which I did not meet with some new experience, some new set of conditions which I had not met with before, and which had then to be met by special and newly devised arrangements.

A well-known English astronomer once told me that he considered a large objective, when finished, as much a work of art as a fine painting.

I have myself always looked upon it less as a mechanical operation than a work of art. It is, moreover, an art most difficult to communicate. It is only to be acquired by some persons, and that after years of toilsome effort, and even the most experienced find it impossible to reduce their method to any fixed rules or formulæ.

[H. G.]

GENERAL MONTHLY MEETING,

Monday, April 5, 1886.

THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. LL.D. President, in the Chair.

John Wolfe Barry, Esq. M. Inst. C.E.
Sir Thomas Brassey, K.C.B. M.P.
Arthur Carpmael, Esq.
Ernest Carpmael, Esq.
Allan Harvey Drummond, Esq.
Edmund Macrory, Esq.
William Hugh Spottiswoode, Esq.

were elected Members of the Royal Institution.

The following Lecture Arrangements were announced:—

PROFESSOR ARTHUR GAMGEE, M.D. F.R.S. Fullerian Professor of Physiology, R.I.—Six Lectures on THE FUNCTION OF CIRCULATION; on Tuesdays, May 4 to June 8.

PROFESSOR DEWAR, M.A. F.R.S. *M.R.I.* Fullerian Professor of Chemistry, R.I.—Three Lectures on THE ALKALOIDS; on Thursdays, May 6 to 20.

PROFESSOR ALEXANDER MACALISTER, M.D. M.A. F.R.S.—Three Lectures on HABIT AS A FACTOR IN HUMAN MORPHOLOGY; on Thursdays, May 27 to June 10.

PROFESSOR ERNST PAUER—Three Lectures on HOW TO FORM A JUDGMENT ON MUSICAL WORKS (with Musical Illustrations): on Saturdays, May 8 to 22.

PROFESSOR GEORGE GABRIEL STOKES, M.A. D.C.L. LL.D. PRES. R.S.—Three Lectures on LIGHT, WITH SPECIAL REFERENCE TO EFFECTS RESULTING FROM ITS ACTION ON VARIOUS SUBSTANCES; on Saturdays, May 29 to June 12.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India. Records, Vol. XIX. Part 1. 8vo. 1885.
Palæontologia Indica; Memoirs, Series X. Vol. III. Parts 7 and 8. fol. 1886.
The Lords of the Admiralty—Diurnal Change in the Magnitude and Direction of the Magnetic Forces in the Horizontal Plane, 1841–76. 4to. 1885.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta; Rendiconti. Vol. II. Fasc. 2–6. 8vo. 1886.
American Academy of Arts and Sciences—Memoirs, Vol. XI. Part 3, Nos. 2, 3. 4to. 1885.
Proceedings, Vol. XXI. Part 1. 8vo. 1885.
American Philosophical Society—Proceedings, No. 121. 8vo. 1886.

Antiquaries, Society of—Proceedings, Vol. X. No. 3. 8vo. 1885.
Asiatic Society, Royal (Bombay Branch)—Journal, Vol. XVI. No. 43. 8vo. 1885.
Astronomical Society, Royal—Monthly Notices, Vol. XLVI. No. 4. 8vo. 1886.
Ateneo Veneto—Revista Mensile. 8vo. 1883-5.
Balfour, The Relations of the late F. M. (the Author)—Works. Memorial Edition. 4 vol. 4to. 1885.
Bankers, Institute of—Journal, Vol. VII. Part 3. 8vo. 1886.
British Architects, Royal Institute of—Proceedings, 1885-6, No. 9-12. 4to.
British Museum (Natural History)—Catalogue of Fossil Mammalia. By R. Lydekker. Part 2. 8vo. 1885.
Catalogue of Palæozoic Plants. By R. Kidston. 8vo. 1886.
Cambridge Philosophical Society—Proceedings, Vol. V. Part 5. 8vo. 1886.
Chemical Society—Catalogue of the Library. 8vo. 1886.
Journal for March, 1886. 8vo.
Chief Signal Officer, U.S. Army—Report, 1884. 8vo.
Report of the International Polar Expedition to Point Barrow, Alaska. 4to. 1885.
Civil Engineers' Institution—Minutes of Proceedings, Vol. LXXXIII. 8vo. 1885-6.
Comptroller of the Currency, U.S.—Annual Report, 1885. 8vo.
Editors—Amateur Photographer for March, 1886. 4to.
American Journal of Science for March, 1886. 8vo.
Analyst for March, 1886. 8vo.
Athenæum for March, 1886. 4to.
Chemical News for March, 1886. 4to.
Engineer for March, 1886. fol.
Horological Journal for March, 1886. 8vo.
Iron for March, 1886. 4to.
Nature for March, 1886. 4to.
Revue Scientifique for March, 1886. 4to.
Telegraphic Journal for March, 1886. 8vo.
Zoophilist for March, 1886. 4to.
Fleming, S. (the Author)—Universal or Cosmic Time. 8vo. 1885.
Franklin Institute—Journal, No. 723. 8vo. 1886.
Geographical Society, Royal—Proceedings, New Series, Vol. VIII. Nos. 3, 4. 8vo. 1886.
Horticultural Society, Royal—Journal, Vol. VII. No. 1. 8vo. 1886.
Johns Hopkins University—Studies in Historical and Political Science, Fourth Series, Nos. 2, 3. 8vo. 1886.
American Journal of Philology, No. 24. 8vo. 1886.
University Circular, No. 47. 4to. 1886.
American Chemical Journal, Vol. VII. No. 6. 8vo. 1886.
Kendal and Dent, Messrs. (the Authors)—Time Chart of the World. 1886.
Lisbon, Sociedade de Geographie—Boletim, 5ª Serie, Nos. 7, 8. 8vo. 1885.
Madden, T. More, M.D. (the Author)—Recent Progress of Obstetric and Gynæcological Medicine. 8vo. 1886.
Manchester Geological Society—Transactions, Vol. XVIII. Parts 14, 15, 16. 8vo. 1886.
Marks, W. D. Esq. (the Author)—Law of Condensation of Steam in Cylinders. And Development of Dynamic Electricity. 8vo. 1886.
Meteorological Office—Monthly Weather Report for Sept. Oct. Nov. 1885. 4to.
Hourly Readings, 1883. Part 3. 4to.
Meteorological Society, Royal—Quarterly Journal, No. 57. 8vo. 1886.
Meteorological Record, No. 19. 8vo. 1885.
Numismatic Society—Chronicle and Journal, 1885, Part 4. 8vo.
Odontological Society of Great Britain—Transactions, Vol. XVIII. Nos. 2 3, 5. New Series. 8vo. 1885-6.
Parker-Rhodes, C. E. Esq. (the Author)—Universal Reading of Time. 12mo. 1885.

Pharmaceutical Society of Great Britain—Journal, March, 1886. 8vo.
Photographic Society—Journal, New Series, Vol. X. No. 5. 8vo. 1886.
Rodd, Major J. R. M.R.I.—Feda, with other Poems. By Rennell Rodd. 12mo. 1886.
Royal Society of London—Proceedings, No. 241. 8vo. 1885.
Saxon Society of Sciences, Royal—Mathematische-Physische Classe: Berichte, 1885. No. 3. 8vo. 1886.
Society of Arts—Journal, March, 1886. 8vo.
St. Pétersbourg, Académie des Sciences—Bulletins, Tome XXX. No. 3. 4to. 1886.
United States Geological Survey—Bulletins, Nos. 7–14. 8vo. 1884–5.
Mineral Resources of the United States, 1883-4. By A. Williams. 8vo. 1885.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1886: Heft 2. 4to.
Vienna—Naturhistorischen Hofmuseums—Annalen. Band I. No. 1. 4to. Wien, 1886.
Wild, Dr. H. (the Director)—Annalen der Physikalischen Central-Observatoriums, 1884. Thiel 2. 4to. 1885.
Zoological Society—Transactions, Vol. XI. Part 11; Vol. XII. Part 1. 4to. 1885–6.

WEEKLY EVENING MEETING,

Friday, April 9, 1886.

Sir William Bowman, Bart. LL.D. F.R.S. Vice-President, in the Chair.

William Anderson, Esq. M. Inst. C.E. *M.R.I.*

On New Applications of the Mechanical Properties of Cork to the Arts.

It would seem difficult to discover any new properties in a substance so familiar as cork, and yet it possesses qualities which distinguish it from all other solid or liquid bodies, namely, its power of altering its volume in a very marked degree in consequence of change of pressure. All liquids and solids are capable of cubical compression, or extension, but to a very small extent; thus water is reduced in volume by only one two-thousandth part by the pressure of one atmosphere. Liquid carbonic acid yields to pressure much more than any other fluid, but still the rate is very small. Solid substances, with the exception of cork, offer equally obstinate resistance to change of bulk, even indiarubber, which most people would suppose capable of very considerable change of volume, we shall find is really very rigid.

I have here an apparatus for applying pressure by means of a lever. I place a piece of solid indiarubber under the plate and you see that I can compress it considerably by a very light pressure of my finger. I slip this same piece of indiarubber into a brass tube, which it fits closely, and now you see that I am unable to compress it by any force which I can bring to bear, I even hammer the lever with a mallet and the blow falls as it would on a stone. The reason of this phenomenon is, that, in the first place, with the indiarubber free, it spread out laterally while being compressed longitudinally, and consequently the volume was hardly altered at all; in the second case, the strong brass tube prevented all lateral extension, and because indiarubber is incapable of appreciable cubical compression, its length only could not be sensibly altered by pressure.

Extension, in like manner, does not alter the volume of indiarubber. In this glass tube is a piece of solid round rubber which nearly fills the bore. The lower end of the rubber is fixed in the bottom of the tube and the upper end is connected by a fine cord to a small windlass, by turning which I can stretch the rubber. I fill the tube to the brim with water and throw an image of it on to the screen. If stretching the rubber either increases or diminishes its volume, the water in the tube will either overflow or shrink in it. I now stretch the rubber to about 3 inches, or one-third of its original length, but you cannot see any appreciable movement in the water-level, hence the volume of the rubber has not changed.

Metals when subjected to pressures which exceed their elastic limits, so that they are permanently deformed, as in forging or wire-drawing, remain practically unchanged in volume per unit of weight.

I have here a pair of common scales. To the under sides of the pans I can hang the various specimens that I wish to examine; underneath these are small beakers of water which I can raise or lower by means of a rack and pinion. Substances immersed in water lose in weight by the weight of their own volume of water; hence if two substances of equal volume balance each other in air, they will also balance when immersed in water, but if their volumes are not the same, then the substance having the smaller volume will sink, because the weight of water it displaces is less than that displaced by the substance with the larger volume. To the scale on your left hand is suspended a short cylinder of ordinary iron, and to the right hand scale a cylinder of ordinary copper. They balance exactly. I now raise the beakers and immerse the two cylinders in water, you see the copper cylinder sinks at once, and I know by that, that copper has a smaller volume per pound than iron, or, as we should commonly say, it is heavier than iron. I now detach the copper cylinder and in its place hang on this iron one, which is made of the same bar as its fellow cylinder, but forced, while red hot, into a mould by a pressure of sixty tons per square inch and allowed to cool under that pressure. The two cylinders balance, as you see. Has the volume of the iron in the compressed cylinder been altered by the rough treatment it has received? I raise the beakers, immerse the cylinders, the balance is not destroyed, hence we conclude that although the form has been changed the volume has remained the same. I substitute for the hot compressed cylinder one pressed into a mould while cold, and held there for some time, with a load of sixty tons per square inch; the balance is not destroyed by immersion, hence the volume has not been altered. I can repeat the experiments with these copper cylinders and the result will be found the same. Extension also is incapable of appreciably altering the density of metals. I attach to the scales two specimens of iron taken from a bar which had been torn asunder by a steady pull. One specimen is cut from the portion where it had not been strained, and the other from the very point where it had been gradually drawn out and fractured. The specimens balance, I immerse them, you see the balance is not destroyed; hence the volume of the iron has not been changed appreciably by extension.

But cork behaves in a very different manner. I place this cylinder of cork into just such a brass tube as served to restrain the indiarubber and apply pressure to it in the same way: you see I can readily compress the cork, and when I release it it expands back to its original volume: the action is a little sluggish on account of the friction of the cork against the sides of the tube. In this case, therefore, a very great change in the volume of the material has been easily effected.

But although solids evidently do not change sensibly in bulk, after having been released from pressures high enough to distort them permanently, yet, while actually under pressure, the volumes may have been considerably altered. As far as I am aware, this point has not been determined experimentally for metals, but it is very easy to show that indiarubber does not change.

I have here some of this substance which is so very slightly lighter than water, that, as you see, it only just floats in cold water but sinks in hot. If I could put it under considerable pressure while afloat in cold water, then, if its volume became sensibly less, it ought to sink. In the same way if I load a piece of cork and a piece of wood so that they barely float, if their volumes alter, they ought to sink.

In this strong upright glass tube, I have, at the top, a piece of indiarubber, immediately below it a piece of wood, and below that a cork, the wood and the cork are loaded with metal sinkers to reduce their buoyancy. The tube is full of water and is connected to a force-pump by means of which I can impose a pressure of over 1000 lbs. per square inch. The image of the tube is now thrown on the screen and the pressure is being applied. You see at once the cork is beginning to shrink in all directions, and now its volume is so reduced that it is incapable of floating and sinks down to the bottom of the tube. The indiarubber is absolutely unaffected, the wood does contract a little, but not sufficiently to be visible to you or to cause it to sink. I open a stop-cock and relieve the pressure, you see that the cork instantly expands, its buoyancy is restored and it floats again. By alternately applying and taking off the pressure I can produce the familiar effect, so well known in the toy called "the bottle imps." It is this singular property which gives to cork its value as a means of closing the mouths of bottles. Its elasticity has not only a very considerable range, but it is very persistent. Thus in the better kind of corks used in bottling champagne and other effervescing wines, you are all familiar with the extent to which the corks expand the instant they escape from the bottles. I have measured this expansion, and find it to amount to an increase of volume of 75 per cent. even after the corks have been kept in a state of compression in the bottles for ten years. If the cork be steeped in hot water, the volume continues to increase till it attains nearly three times that which it occupied in the neck of the bottle.

When cork is subjected to pressure, either in one direction, as in this lever press, or from every direction, as when immersed in water under pressure, a certain amount of permanent deformation or "permanent set" takes place very quickly. This property is common to all solid elastic substances when strained beyond their elastic limits, but with cork the limits are comparatively low. You have, no doubt, noticed in chemists' and other shops, that when a cork is too large to fit a bottle, the shop-keeper gives the cork a few sharp bites, or, if he be more refined, he uses a pair of specially contrived pincers, in either case he squeezes the cork beyond its elastic limits and so makes it permanently smaller. Besides the permanent set, there is a certain amount of what I venture to call sluggish elasticity, that is, cork on being released from pressure, springs back a certain amount at once, but the complete recovery takes an appreciable time.

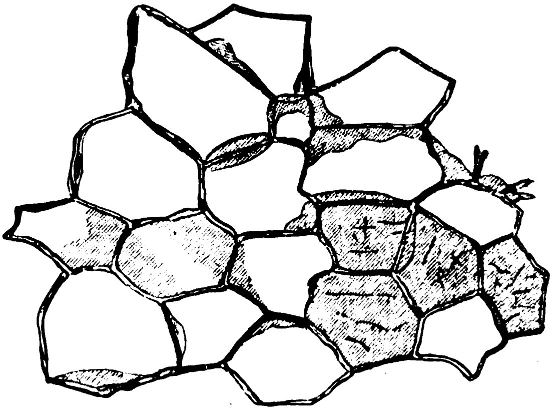
While I have been speaking, a piece of fresh cork, loaded so as barely to float, has been inserted into the vertical glass pressure-tube. I apply a slight pressure, you see the cork sinks. I release the pressure, and it rises briskly enough. I now apply a much higher pressure for a moment or two, I release it, and the cork will either not rise at all, or will do so very slowly; its volume has been permanently altered; it has taken a permanent set.

In considering the properties of most substances, our search for the cause of these properties is baffled by our imperfect powers and the feeble instruments we possess for investigating molecular structure. With cork, happily, this is not the case; an examination of its structure is easy, and perfectly explains the cause of its peculiar and valuable properties.

All plants are built up of minute cells of various forms and dimensions. Their walls or sides are composed chiefly of a substance called cellulose, frequently associated with lignine, or woody matter, and with cork, which last is a nitrogenous substance found in many portions of plants, but is especially developed in the outer bark of exogenous trees, that is, trees belonging to an order, by far the most common in these latitudes, the stems of which grow by the addition of layers of fresh cellulose tissue outside the woody part and inside the bark. Between the bark and the wood is interposed a thin fibrous layer, which, in some trees, such as the lime, is very much developed, and supplies the bass matting with which all are familiar. The corky part of the bark, which is outside, is composed of closed cells exclusively, Figs. 1 and 2, so built together that no connection of a tubular nature runs up and down the tree, although horizontal passages radiating towards the woody part of the tree are numerous. In the woody part of the tree, on the contrary, and in the inner bark, vertical passages or tubes exist, while a connection is kept up with the pith of the tree by means of medullary rays. In one species of tree, known as the cork oak, the corky part of the bark is very strongly developed. I project on the screen the magnified image of a horizontal section of the bark of the cork oak; you see nine or ten bands running parallel to each other, these are the layers of cellulose matter that have been deposited in successive years. I turn the specimen, and you now see the vertical section with the radiating passages clearly marked.

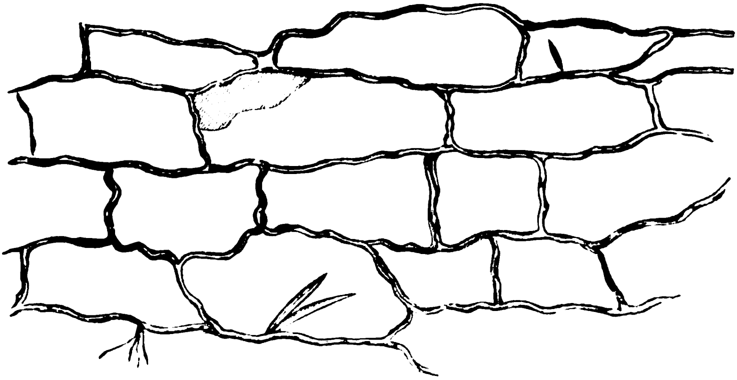
The difference between the arrangement of the cells or tissue forming the woody part of the tree and the bark is easily shown. I have here three metal sockets, supported over a shallow wooden tray. Into them are fitted, first, a cork cut out of the bark in a vertical direction, next, a cork cut in a radial direction, and, lastly, a piece of common yellow pine. By means of my force-pump, I apply a couple of atmospheres of hydraulic pressure. I project an image of the apparatus on the screen, and you see the water has made its way through the wood and through the cork cut in the radial direction, while the cork cut in the vertical direction is impervious.

FIG. 1.



Vertical Section.

FIG. 2.



Horizontal Section.

Magnified about 300 diameters.

The cork tree, a species of evergreen oak, is indigenous in Portugal and along both shores of the Mediterranean. The diagram on the wall has been painted from a sketch obligingly sent to me by Mr.

C. A. Friend, the Resident Engineer of the Seville Waterworks, to whom I am also indebted for this branch of a cork tree, these acorns, this axe used in getting the cork, and for a description of the habits of the tree, its cultivation, and the mode of gathering the harvest.

The cork oak attains a height of thirty to forty feet; it is not cultivated in any way, but grows like trees in a park. The first crop is not gathered till the tree is thirty years old, the next nine or ten years later;—both these crops yield inferior cork, but at the third crop, gathered when the tree is fifty years old, the bark has attained full maturity, and after that will yield the highest quality of cork every nine or ten years. In the autumn of the year, when the bark is in a fit state, that is, for small trees, from three-quarters of an inch to one inch thick, and for larger ones up to one and a half inch, a horizontal cut is made, by means of a light axe like the one I hold in my hand, through the bark a few inches above the ground; succeeding cuts are made at distances of about a yard, up to the branches, and even along some of the large ones, then two or more vertical cuts, according to the size of the tree, and the bark is ripped off by inserting the wedge-shaped end of the axe-handle. In making the cuts great care is taken to avoid wounding the inner bark, upon the integrity of which the health of the tree depends; but where this precaution is taken, the gathering of the cork does not in any way injure the tree.

After stripping, the cork is immersed for about an hour in hot water, it is dressed with a kind of spokeshave, then laid out flat and weighted in order to take out the curvature; it is then stacked in the open air, without protection of any kind, for cork does not appear to be susceptible of receiving injury from the weather.

The minute structure of the bark is very remarkable. First, I project on the screen a microscopic section of the wood of the cork tree. It is taken in a horizontal plane, and I ask you to notice the diversity of the structure, and especially the presence of large tubes or pipes. I next exhibit a section taken in the same plane of the corky portion of the bark, Fig. 2. You see the whole substance is made up of minute many-sided cells about $\frac{1}{750}$ inch in diameter and about twice as long, the long way of the cells being disposed radially to the trunk. The walls of the cells are extremely thin, and yet they are wonderfully impervious to liquids. Looked at by reflected light, if the specimen be turned, bands of silvery light alternate with bands of comparative darkness, showing that the cells are built on end to end in regular order. The vertical section next exhibited, Fig. 1, shows a cross-section of the cells looking like a minute honey-comb. In some specimens large numbers of crystals are found. These could not be distinguished from the detached elementary spindle-shaped cells, of which woody fibre is made up, were it not for the powerful means of analysis we have in polarized light. I need hardly explain to an audience in this Institution that light passed through a Nicol prism becomes polarized, that is to say, the vibrations of the luminiferous ether are all reduced to vibrations in one plane, and, consequently, if a second prism be interposed and placed at right angles to the first, the light will be unable to get through; but if we introduce between the crossed Nicols a substance capable of turning the plane of vibration again, then a certain portion of the light will pass. I have now projected on the screen the feeble light emerging from the crossed Nicols. I introduce the microscopic preparation of cork cells between them, and you see the crystals glowing with many-coloured lights on a dark ground.

Minute though these crystals are, they are very numerous and hard, and it is partly to them that is due the extraordinary rapidity with which cork blunts the cutting instruments used in shaping it. Cork-cutters always have beside them a sharpening stone, on which they are obliged to restore the edges of their knives after a very few cuts.

The cells of the cork are filled with gaseous matter, which is very easily extracted, and which has been analysed for me by Mr. G. H. Ogston, and proved to be common air. I have here a glass tube in which are some pieces of cork which have been cut into slices so as to facilitate the escape of the air. I connect the tube with an exhausted receiver and project the image on the screen; you see rising from the cork bubbles of air as numerous, but much more minute than the bubbles which rise from sparkling wines; much more minute, because the bubbles you see are expanded to seven or eight times their volume at atmospheric pressure, on account of the vacuum existing in the tube. The air will continue to come off for an hour or more, and from measurements made by Mr. Ogston I find that the air occluded in the cork amounts to about 53 per cent. of its volume. The facility with which the air escapes, compared with the impermeability of cork to liquids, is very remarkable.

I throw on the screen the image of a section cut from a cork which was kept under a vacuum of about 26 inches for five days and nights; aniline dye was then injected, and yet you see that the colour has not more than permeated the outermost fringe of cells, those, in fact, which had been broken open by the operation of cutting the cork. By keeping cork for a very long time in an almost perfect vacuum, and then injecting dye, a slight darkening of the general colour of a section of the cork may be noticed, but it is very slight indeed. How, then, does the air escape so readily when the cork is placed in vacuo?

The answer is, that gases possess the property of diffusion, that is, of passing through porous media of inconceivable fineness. When two gases, such as hydrogen and air, are separated by a porous medium, they immediately begin to pass into each other, and the lighter gas passes through more quickly than the heavier.

I have here a glass tube, the upper end of which is closed by a thin slice of cork, the lower end dips into a basin of water. Some hours ago the tube was filled with hydrogen, which you know is about fourteen and a half times lighter than air, consequently, according to the law of diffusion, it will get out of the tube through the cork quicker than the air can get in by the same means, and the result must be that a partial vacuum will be formed in the tube, and a column of water will be drawn up. You see that such has been the case, and we have thus proved that the cells of cork are eminently pervious to gases. The pores in the cell-walls appear, however, to be too minute to permit the passage of liquids.

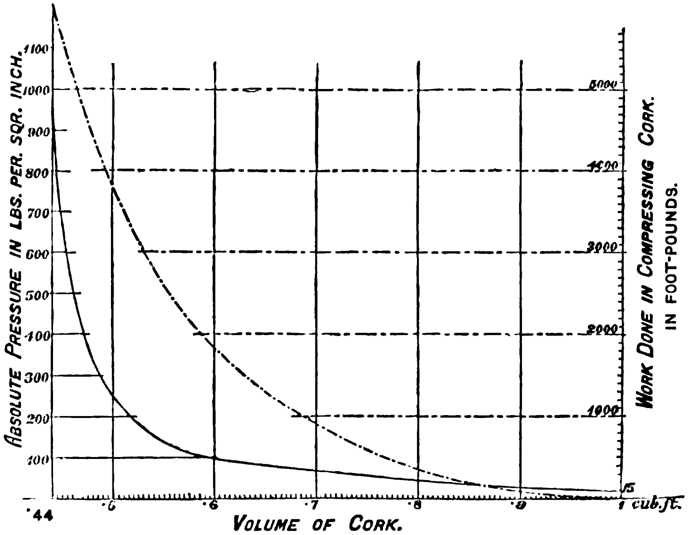
I closed the end of a glass tube 11 mm. diameter, with a disc of cork 1·75 mm. thick, cut at right angles to the axis of the tree, I placed a solution of blue litmus inside the tube, and suspended it in a weak solution of sulphuric acid. Had diffusion taken place, both liquids would have assumed a red colour, but after sixteen hours no change whatever could be detected. A like inertness was exhibited when the tube was filled with a solution of copper sulphate and suspended in a weak solution of ammonia; a deep blue colour would have appeared had any intermixture taken place, and the same tube is before you immersed in ammonia and filled with red litmus solution. It has been in this condition since the 28th of February, but no diffusion has taken place. A disc of wood 6 mm. thick under the same circumstances showed, after a couple of hours, by the liquids turning blue, that diffusion was going on actively. It is this property of allowing gases to permeate while completely barring liquids that enables cork to be kept in compression under water or in contact with various liquids without the air-cells becoming water-logged, and that makes cork so admirable an article for waterproof wear, such as boot soles and hats, for, unlike indiarubber, it allows ventilation to go on while it keeps out the wet. The cell-walls are so strong, notwithstanding their extreme thinness, that they appear, when empty, to be able to resist the atmospheric pressure, for the volume of the cork does not sensibly diminish, even when all the air has been extracted. Viewed under very high power, cross-stays or struts of fibrous matter may be distinguished traversing the cells: these, no doubt, add to the strength and resistance of the structure.

From what you have seen you will have no difficulty in arriving at the conclusion that cork consists, practically, of an aggregation of minute air-vessels, having very thin, very water-tight, and very strong walls, and hence, if compressed, we may expect the resistance to compression to rise in a manner more like the resistance of gases than the resistance of an elastic solid such as a spring. In a spring the pressure increases in proportion to the distance to which the spring is compressed, but with gases, the pressure increases in a much more rapid manner, that is, inversely as the volume which the gas is made to occupy. But from the permeability of cork to air, it is evident that if subjected to pressure in one direction only, it will gradually part with its occluded air by effusion, that is by its passage through the porous walls of the cells in which it is contained. This fact can be readily demonstrated by the lever press which I have used, for if the brass cylinder containing the cork be filled with soap and water and pressure be then applied, minute bubbles will be found to collect on the surface, and their formation will go on for many hours.

On the other hand, if cork be subjected to pressure from all sides, such as operates when it is immersed in water under pressure, then the cells are supported in all directions, the air in them is reduced in volume, and there is no tendency to escape in one direction more than another. An indiarubber bag, such as this, distended by air, bursts, as you see, if pressed between two surfaces, but if an indiarubber cell be placed in a glass tube and subjected to hydraulic pressure, it is merely shrivelled up, the strain on its walls is actually reduced.

To take advantage of the peculiar properties of cork in mechanical applications, it is necessary to determine accurately the law of its resistance to compression, and for this purpose I instituted a series of experiments of this kind. Into a strong iron vessel of 5½ gallons capacity I introduced a quantity of cork, and filled the interstices full of water, carefully getting out all the air. I then proceeded to pump in water, until definite pressures up to 1000 pounds per square inch had been reached, and, at every 100 pounds, the weight of water pumped in was determined. In this way, after many repetitions, I obtained the decrease of volume, due to any given increase of pressure. The observations have been plotted into the form of a curve, Fig. 3, which you see on the diagram on the wall. The base-line represents a cylinder containing one cubic foot of cork divided by the vertical lines into ten parts; the black horizontal lines according to the scale on the left hand represent the pressures in pounds per square inch

Fig. 3.



which were necessary to compress the cork to the corresponding volume. Thus to reduce the volume to one-half, required a pressure of 250 pounds per square inch. At 1000 pounds per square inch the volume was reduced to 44 per cent., the yielding then became very little, showing that the solid parts of the cells had nearly come together, and this corroborates Mr. Ogston's determination, that the gaseous part of cork constitutes 53 per cent. of its bulk. The engineer, in dealing with a compressible substance, requires to know not only the pressure which a given change of volume produces, but also the work which has to be expended in producing the change of volume. The work is calculated by multiplying the decrease of volume by the mean pressure per unit of area which produced it. The ordinates of the dotted curve on the diagram with the corresponding scale of foot pounds on the right-hand side are drawn equal to the work done in compressing a cubic foot of cork to the several volumes marked on the base line. I have not been able to find an equation to the pressure curve, it seems to be quite irregular, and hence the only way of calculating the effects of any given change of volume is to measure the ordinates of the curve constructed by actual experiment. As may be supposed the pressures indicated by experiment are not nearly so regular and steady as corresponding experiments in a gas would be, and the actual form of the curves will depend on the quality of the cork experimented on.

The last point of importance in this inquiry relates to the permanence of elasticity in cork.

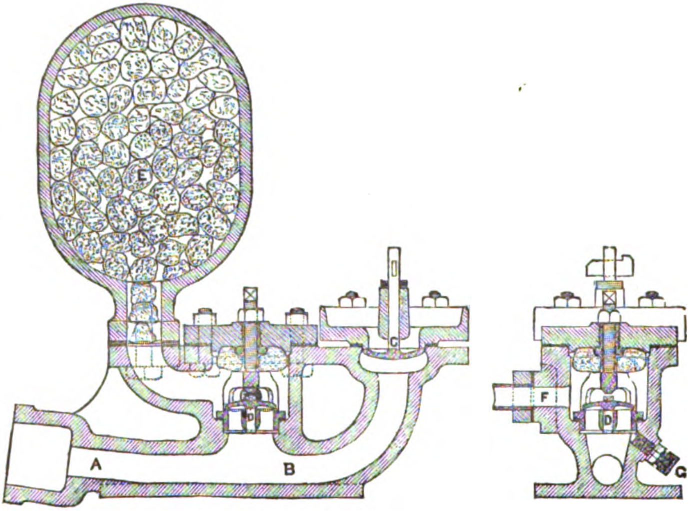
So far as preservation of elasticity during years of compression is concerned, we have the evidence of wine corks to show that a considerable range of elasticity is retained for a very long time. With respect to cork subjected to repeated compression and extension, I have very little evidence to offer beyond this, that cork which had been compressed and released in water many thousand times, had not changed its molecular structure in the least, and had continued perfectly serviceable. Cork which has been kept under a pressure of three atmospheres for many weeks appears to have shrunk to from 80 to 85 per cent. of its original volume.

I will conclude this lecture by bringing under your notice two novel applications of cork to the arts.

Before the lecture-table stands a water-raising apparatus called a hydraulic ram. The structure of the machine is shown by a diagram on the wall, Fig. 4. The ram consists of an inclined pipe A, which leads the water from a reservoir into a chamber B, which terminates in a valve C, opening inwards. Branching up from the chamber is a passage leading to a valve D, opening outwards and communicating with a regulating vessel E, which is usually filled with air, but which I prefer to fill with cork and water. Immediately beyond the inner valve, is inserted a delivery pipe F, which is laid to the spot to which the water has to be pumped, in this case to the fountain jet in the middle of this pan.

The action of the ram is as follows:—The outer valve C which opens inwards, is, in the first instance, held open, and a flow of water is allowed to take place through it down the pipe and chamber. The valve is then released, and is instantly shut by the current of water which is thus suddenly stopped, and, in consequence, delivers a blow similar to that produced by the fall of a hammer on an anvil, and just as the hammer jumps back from the anvil, so does the water recoil back to a small extent along the pipe.

FIG. 4.



During this action, first, a certain portion of water is forced by virtue of the blow through the inner valve D, opening outwards, into the cork vessel, and so to the delivery pipe, and, instantly afterwards the recoil causes a partial vacuum to form in the body of the ram and permits the atmospheric pressure to open the outer valve C and re-establish a rush of water as soon as the recoil has expended itself. In the little ram before you, this action, which it has taken so long to describe, is repeated 140 times in a minute.

The ram is now working, you hear the regular pulses of the valve, and you see a jet of water rising some 10 feet into the air. I throw the electric light on the water, and I ask you to notice the regularity of the flow. You can, indeed, detect the pulses of the ram in the fountain, but that is because I am only using a regulating vessel of the same capacity as that generally used for air, and you will recollect that 44 per cent. of the substance of cork is solid and inelastic. By closing a cock, I can cut off the cork vessel from the ram, you see the regularity of the jet has disappeared, it now goes in leaps and bounds. This demonstrates that the elasticity of cork is competent to regulate the flow of water. When air is used for this purpose, the air-vessel has to be filled, and, with most kinds of water, the supply has to be kept up while the ram is working, because water under pressure absorbs air. For this purpose a "sniff-valve," G, is a necessary part of all rams. It is a minute valve opening inwards, placed just below the inner valve; at each recoil, a small bubble of air is drawn in and passed into the air-vessel. This "sniff-valve" is a fruitful source of trouble. Its minuteness renders it liable to get stopped up by dirt; it must not, of course, be submerged, and, if too large, it seriously affects the duty performed by the ram. The use of cork gets rid of all these difficulties, no sniff-valve is needed, the ram will work deeply submerged, and there is no fear of the cork vessel ever getting empty. The duty which even the little ram before you has done is 65 per cent., and larger ones have reached 80 per cent.

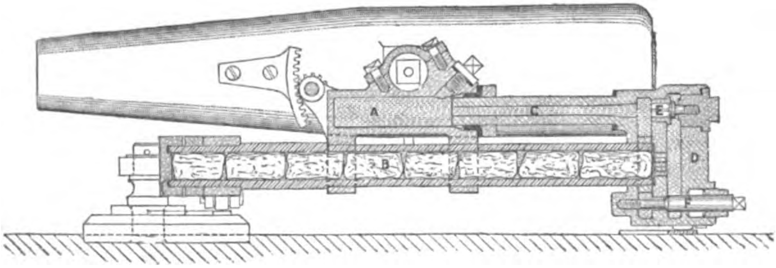
The second novel application of cork is, for the purpose of storing a portion of the energy of the recoil of cannon, for the purpose of expending it afterwards in running them out.

The result of the explosion of gunpowder in a gun is to drive the shot out in one direction, and to cause the gun to recoil with equal energy the opposite way. To restrain the motion of the gun, "compressors" of various kinds are used, and in this country, for modern guns, they are generally hydraulic, that is to say, the force of recoil is expended in causing the gun to mount an inclined plane, and, at the same time, in driving a piston into a cylinder full of water, the latter being allowed to squeeze past the piston through apertures, the areas of which are either fixed, or capable of being automatically varied as the gun recedes; or else the water is driven out of the cylinder through loaded valves. As a rule, the gun is moved out again into its firing position by its weight, causing it to run down the inclined plane, up which it had previously recoiled. For naval purposes, however, this plan is inconvenient, because the gun will not run out to windward if the vessel is heeling over, on account of the inclined plane becoming more horizontal, or even inclined in the reverse direction, and should the ship take a permanent list, from a compartment getting full of water, the inconvenience might be very considerable.

In land service guns, when mounted in barbette, the rising of the gun exposes it and the loading detachment more to the enemy's fire, and in both cases, when placed in ports or embrasures, the ports must be higher than if the gun recoiled horizontally, and will therefore offer a better mark to the enemy's fire, especially that of machine guns, while the sudden rise of the gun in recoiling imposes a severe downward pressure on the deck or on the platform.

To obviate these disadvantages I have contrived the gun-carriage, a model of which is before you on the table, and a diagram of which, Fig. 5, on the wall illustrates the internal construction. The gun is mounted on a carriage composed of two hydraulic cylinders A, united so as to form one piece. This carriage slides on a pair of hollow ways B, and also on to a pair of fixed rams C, the rear ends of which are attached to the piece D forming the rear of the mounting. There are water passages down the axes of the rams, and these communicate through an automatic recoil valve E, opening from the cylinders, with the two hollow slides B. There is a second communication, between the cylinders and slides by means of a cock F, which can be opened or shut at pleasure. The hollow slides are packed full of cork and water, the latter also completely filling the cylinders, rams, and various connecting passages.

Fig. 5.



By means of a small force-pump enough water can be injected to give the cork so much initial compression as will suffice to run the gun out when the slides are inclined under any angle which may be found convenient.

When the gun is fired, the cylinders A are driven on to the rams C and the water in the cylinders is forced through the hollow rams into the cork and water vessels formed by the slides B, and the cork is compressed still farther. When the recoil is over, the automatic recoil-valve E closes, and the gun remains in its rearward position ready for loading.

As soon as loaded, the running-out cock F is opened, the expansion of the cork drives the water from around it into the cylinders, and so forces the gun out.

If it be desired to let the gun run out automatically immediately after recoil, it is only necessary to leave the running-out cock F open, and then the water forced among the cork by recoil returns instantly to the cylinders, and runs the gun out quicker than the eye can follow the motion.

I will now load the model and fire a shot into this strong steel cylinder, at the bottom of which is a thick layer of soft wood. I will close the running-out valve, so that the gun shall remain in the recoiled position. Sir Frederick Abel has kindly arranged some of his electric fuses specially to fit this minute ordnance, and I can fire the gun by means of a small electro-magnetic battery. The gun has now recoiled, and remains in its rear position. I load again, open the running-out cock, the gun runs out, and I fire without closing the cock. You see the gun has recoiled and run out instantly again.

The arrangement I have adopted may be made by using air instead of cork, but air is a troublesome substance to deal with; it leaks out very easily and without showing any signs of having done so, which might readily lead to serious consequences. A special pump is required to make up loss by leakage.

The merit of cork is its extreme simplicity and trustworthiness. By mixing a certain proportion of glycerine with the water it will not freeze in any ordinary cold weather.

[W. A.]

WEEKLY EVENING MEETING,

Friday, April 16, 1886.

Sir William Bowman, Bart. LL.D. F.R.S. Vice-President, in the Chair.

Professor Sir Henry E. Roscoe, M.P. LL.D. F.R.S.

On Recent Progress in the Coal-tar Industry.

Those who have read Goethe's episodes from his life, known as 'Wahrheit und Dichtung,' will remember his description of his visit in 1741 to the burning hill near Dutweiler, a village in the Palatinate. Here he met old Stauf, a coal philosopher, *philosophus per ignem*, whose peculiar appearance and more peculiar mode of life, Goethe remarks upon. He was engaged in an unsavoury process of collecting the oils, resin, and tar, obtained in the destructive distillation of coal carried on in a rude form of coke oven. Nor were his labours crowned with pecuniary success, for he complained that he wished to turn the oil and resin into account, and save the soot, on which Goethe adds that in attempting to do too much, the enterprise altogether failed. We can scarcely imagine, however, what Goethe's feelings would have been could he have foreseen the beautiful and useful products which the development of the science of a century and a half has been able to extract from Stauf's evil smelling oils. With what wonder would he have regarded the synthetic power of modern chemistry, if he could have learnt that not only the brightest, the most varied colours of every tone and shade can be obtained from this coal tar, but that some of the finest perfumes can, by the skill of the chemist, be extracted from it. Nay, that from these apparently useless oils, medicines which vie in potency with the rare vegeto-alkaloids can be obtained, and lastly, perhaps most remarkable of all, that the same raw material may be made to yield an innocuous principle, termed *saccharine*, possessed of far greater sweetness than sugar itself. The attainment of such results might well be regarded as savouring of the chimerical dreams of the alchemist, rather than expressions of sober truth, and the modern chemist may ask a riddle more paradoxical than that of Samson, "Out of the burning came forth coolness, and out of the strong came forth sweetness"; and by no one could the answer be given who had not ploughed with the heifer of science, "What smells stronger than tar and what tastes sweeter than saccharine?" That these are matters of fact we may assure ourselves by the most convincing of all proofs—their money value, and we learn that the annual value of the products now extracted from an unsightly and apparently worthless material, amounts to several millions sterling, whilst the industries based upon these results give employment to thousands of men.

Sources of the Coal-tar products.—In order to obtain these products, whether colours, perfumes, antipyretic medicines, or sweet principle, a certain class of raw material is needed, for it is as impossible to get nutriment from a stone as to procure these products from wrong sources. All organic compounds can be traced back to certain hydrocarbons, which may be said to form the skeletons of the compounds, and these hydrocarbons are divisible into two great classes: (1) the paraffinoid, and (2) the benzenoid hydrocarbons. The chemical differences both in properties and constitution between these two series are well marked. One is the foundation of the fats, whilst the other class gives rise to the essences or aromatic bodies. Now all the colours, finer perfumes, and antipyretic medicines referred to, are members of the latter of these two classes. Hence if we wish to construct these complicated structures, we must employ building materials which are capable of being cemented into a coherent edifice, and therefore we must start with hydrocarbons belonging to the benzenoid series, as any attempt to build up the colours directly from paraffin compounds would prove impracticable. Of all the sources of hydrocarbons, by far the largest is the natural petroleum oils. But these consist almost entirely of paraffins, and hence this source is commercially inapplicable for the production of colours. We have, however, in coal itself, a raw material which by suitable treatment may be made to yield oils of a valuable character. Of these treatments, that followed out in the process of gas-making is the most important, for in addition to illuminating gas in abundant supply, tar is produced which contains principally that benzenoid class of substances already referred to, and which, to use the words of Hofmann, "is one of the most wonderful productions in the whole range of chemistry." The production of these latter as distinguished from the paraffinoid group appears to depend upon a high temperature being employed, to effect the necessary decomposition.

The quantity of coal made into coke for use in the blast furnace is larger than that distilled for gas-making, no less than between eleven and twelve million tons of coal being annually consumed in the blast furnaces of this country in the form of coke, and capable of yielding two million tons of volatile products. Up to recent times, however, the whole of these volatile products has been burnt and lost in the coke ovens. But lately, various processes have been devised for preventing this loss, and for obtaining the oils, which might be made available as colour-producing materials. It is, moreover, a somewhat remarkable fact that only in one or two cases have the conditions been complied with which render it possible to obtain the necessary benzenoid substances. In the ordinary coking ovens, as well as in the blast furnaces, although the temperature ultimately reached is far in excess of that needed to form the colour-giving hydrocarbons, yet the heating process is carried on so gradually that the volatile products from the coal are obtained in the form of paraffinoid bodies mainly, and hence are useless for colour-making purposes. Amongst the few coking processes in which the heat is suddenly applied, and consequently a yield of colour-giving hydrocarbons is obtained, may be mentioned the patented process of Simon-Carvès, the use of which is now spreading in England and abroad. The tar obtained in this process is almost identical in composition with the average gas-works tar, whilst the coke also appears to be equal for iron-smelting purposes to that derived from other coke-ovens. A third source of these oils yet remains to be mentioned, viz. those obtained as a by-product in blast furnaces fed with coal.

Another condition has, in addition, to be considered in this industry, and that is the nature of the coal employed for distillation. It is a well-known fact that if Lancashire cannel be exclusively employed in gas-making a highly luminous gas is obtained, but the tar is too rich in paraffins to be a source of profit to the tar-distiller, whilst, on the other hand, coal of a more anthracitic character, like that from Newcastle or Staffordshire, yields a tar too rich in one constituent, viz. naphthalene, and too poor in another, viz. benzene. It is also known to those engaged in carbonising coal principally for the sake of the tar that the coal from different measures, even in the same pit, yields tars of very different constitution. That under these varying conditions products of varying composition are obtained is a result that will surprise no one who considers the complicated chemical changes brought about in the process of the destructive distillation of coal.

History of Benzene and its derivatives.—Having thus sketched the principles upon which the formation of these valuable tar colours depends, we should do wrong to pass over the history of the discovery of benzene (C_6H_6), which contributed so much to the unlocking of the coal-tar treasury.

Faraday in 1825 discovered two new hydrocarbons in the oils obtained from portable gas. One of these was found to be butylene (C_4H_8); to the other Faraday gave the name of bicarburet of hydrogen, as he ascertained its empirical formula to be C_2H ($C = 6$). By exploding its vapour with oxygen, he observed that one volume contains 36 parts by weight of carbon to 3 parts by weight of hydrogen, and its specific gravity compared with hydrogen is therefore 39.*

Mitscherlich, in 1834, obtained the same hydrocarbon by distillation of benzoic acid, $C_7H_6O_2$, with slaked lime, and termed it benzin. He assumed that it is formed from benzoic acid simply by removal of carbon dioxide. Liebig denied this, adding the following editorial note to Mitscherlich's memoir:—"We have changed the name of the body obtained by Professor Mitscherlich by the dry distillation of benzoic acid and lime, and termed by him benzin, into benzol, because the termination 'in' appears to denote an analogy between strychnine, quinine, &c., bodies to which it does not bear the slightest resemblance, whilst the ending in 'ol' corresponds better to its properties and mode of production. It would have been perhaps better if the name which the discoverer, Faraday, had given to this body had been retained, as its relation to benzoic acid and benzoyl compounds is not any closer than it is to that of the tar or coal from which it is obtained."

---

* 'Phil. Trans.,' 1825, p. 440.

Almost at the same time Péligot found that the same hydrocarbon occurs, together with benzone, $C_{13}H_{10}O$ (diphenylketone $CO(C_6H_5)_2$), in the products of the dry distillation of calcium benzoate.

The different results obtained by Mitscherlich and Péligot are represented by the following formulæ:—

$$C_7H_6O_2 + CaO = C_6H_6 + CaCO_3.$$
$$(C_7H_5O_2)_2Ca = C_{13}H_{10}O + CaCO_3.$$

Péligot obtained benzene only as a by-product, exactly as in the preparation of acetone (dimethylketone) from calcium acetate, a certain quantity of marsh gas is always formed.

It is not clear how Liebig became acquainted with the fact that benzene is formed by the dry distillation of coal, as his pupil Hofmann, who obtained it in 1845 from coal-tar, observes: "It is frequently stated in memoirs and text-books that coal-tar oil contains benzene. I am, however, unacquainted with any research in which this question has been investigated." It is, however, worthy of remark that about the year 1834, at the time when Mitscherlich had converted benzene into nitrobenzene, the distillation of coal-tar was carried out on a large scale in the neighbourhood of Manchester; the naphtha which was obtained was employed for the purpose of dissolving the residual pitch, and thus obtaining black varnish. Attempts were made to supplant the naphtha obtained from wood-tar, which at that time was much used in the hat factories at Gorton, near Manchester, for the preparation of "lacquer," by coal-tar naphtha. The substitute, however, did not answer, as the impure naphtha left, on evaporation, so unpleasant a smell, that the workmen refused to employ it. It was also known about the year 1838, that wood-naphtha contained oxygen, whilst that from coal-tar did not, and hence Mr. John Dale attempted to convert the latter into the former, or into some similar substance. By the action of sulphuric acid and potassium nitrate, he obtained a liquid possessing a smell resembling that of bitter almond oil, the properties of which he did not further investigate. This was, however, done in 1842 by Mr. John Leigh, who exhibited considerable quantities of benzene, nitrobenzene, and dinitrobenzene, to the Chemical Section of the British Association meeting that year in Manchester. His communication is, however, so printed in the Report, that it is not possible from the description to identify the bodies in question.

Large quantities of benzene were prepared in 1848, under Hofmann's direction, by Mansfield, who proved that the naphtha in coal-tar contains homologues of benzenes, which may be separated from it by fractional distillation. On the 17th of February, 1856, Mansfield was occupied with the distillation of this hydrocarbon, which he foresaw would find further applications, for the Paris Exhibition, in a still. The liquid in the retort boiled over and took fire, burning Mansfield so severely that he died in a few days.

The next step in the production of colours from benzene and toluene is the manufacture of nitrobenzene, $C_6H_5NO_2$ and nitrotoluene, $C_7H_7NO_2$. The former compound, discovered in 1834 by Mitscherlich, was first introduced as a technical product by Collas under the name of artificial oil of bitter almonds, and Mansfield in 1847 patented a process for its manufacture. It is now used for perfuming soap, but mainly for the manufacture of aniline ($C_6H_5NH_2$) for aniline blue and aniline black and for magenta. It is made on a very large scale by allowing a mixture of well-cooled fuming nitric acid and strong sulphuric acid to run into benzene contained in cast-iron vessels provided with stirrers.

To prepare aniline from nitrobenzene, this compound is acted upon with a mixture of iron turnings and hydrochloric acid in a cast-iron vessel. Commercial aniline is a mixture of this compound with toluidine obtained from toluene contained in commercial benzene. Some idea of the magnitude of this industry may be gained from the fact that in one aniline works near Manchester no less than 500 tons of this material are manufactured annually. From the year 1857, after Perkin's celebrated discovery * of the aniline colours, up to the present day, the history of the chemistry of the tar products has been that of a continued series of victories, each one more remarkable than the last.

Coal-tar Colours.—To even enumerate the different chemical compounds which have been prepared during the last thirty years from coal-tar would be a serious task, whilst to explain their constitution and to exhibit the endless variety of their coloured derivatives which are now manufactured would occupy far more time than is placed at my disposal. Of the industrial importance of these discoveries, the speaker reminded his audience of the wonderful potency of chemical research, as shown by the fact that the greasy material which in 1869 was burnt in the furnaces or sold as a cheap waggon grease at the rate of a few shillings a ton, received two years afterwards, when pressed into cakes, a price of no less than one shilling per pound, and this revolution was caused by Gräbe and Liebermann's synthesis of alizarin, the colouring matter of madder,† which is now manufactured from anthracene at a rate of more than two millions sterling per annum; and it is stated that an offer was once made, in the earlier stages of its history, by a manufacturer of anthracene to the Paris authorities to take up the asphalt used in the streets for the purpose of distilling it, in order to recover the crude anthracene.

---

* See Lectures by Professor Hofmann, F.R.S., 'On Mauve and Magenta,' April 11, 1862, and W. H. Perkin, F.R.S., 'On the Newest Colouring Matters,' May 14, 1869, Proc. Roy. Inst.; also President's Address (Dr. Perkin, F.R.S.), 'Journal of Society of Chemical Industry,' Vol. IV., July 1884, on Coal Tar Colours.

† 'On the Artificial Production of Alizarine, the Colouring Matter of Madder,' by Prof. H. E. Roscoe, Proc. Roy. Inst., April 1, 1870; also Dr. Perkin, F.R.S., 'On the History of Alizarine,' Journal Society of Arts, May 30, 1879.

Again, we have in the azo-scarlets derived from naphthalene a second remarkable instance of the replacement of a natural colouring matter, that of the cochineal insect, by artificial tar-products, and the naphthol-yellows are gradually driving out the dyes obtained from wood extracts and berries. It is, however, true that some of the natural dye-stuffs appear to withstand the action of light better than their artificial substitutes, and our soldiers' red coats are still dyed with cochineal.

The introduction of these artificial scarlets has, it is interesting to note, greatly diminished the cultivation of cochineal in the Canaries, where, in its place, tobacco and sugar are now being largely grown.

Let us next turn to inquire as to the quantities of these various products obtainable by the distillation of one ton of coal in a gas-retort. The six most important materials found in gas-tar from which colours can be prepared, are:—

1. Benzene.
2. Toluene.
3. Phenol.
4. Metaxylene (from solvent naphtha).
5. Naphthalene.
6. Anthracene.

The average quantity of each of these six raw materials obtainable by the destructive distillation of one ton of Lancashire coal is seen in Table I. Moreover, this table shows the average amount of certain colours which each of these raw materials yields, viz.:—

1.
2. } Magenta 0·623 lb.
3. Aurin 1·2 lb.
4. (*Xylidine* 0·07 lb.)
5. Vermilline scarlet 7·11 lbs.
6. Alizarin 2·25 lbs. (20%).

Further it shows the dyeing power of the above quantities of each of these colours, all obtained from one ton of coal, viz.:—

1 and 2. Magenta, 500 yards of flannel.
3. Aurin, 120 yards of flannel 27 in. wide.
4.
5. } Vermilline scarlet, 2560 yards of flannel.
6. Alizarin, 255 yards Turkey red cloth.

Lastly, to point out still more clearly these relationships, the dyeing-power of one pound of coal is seen in the lowest horizontal column, and here we have a particoloured flag, which exhibits the exact amount of colour obtainable from one pound of Lancashire coal.

Let us moreover remember, in this context, that no less than ten million tons of coal are used for gas-making every year in this country, and then let us form a notion of the vast colouring power which this quantity of coal represents.

TABLE I.—ONE TON OF LANCASHIRE COAL YIELDS WHEN DISTILLED IN GAS RETORTS ON AN AVERAGE

Gas (cubic feet).	Ammoniacal Liquor, 5° Tw.	Equal to Ammonium Sulphate.	Coal (Gas) Tar, sp. gr. 1·16.	Coke.
10,000	20 to 25 gallons.	30 lbs.	12 gallons = 139·2 lbs.	13 hundredweights.

TWELVE GALLONS OF GAS TAR YIELD (Average of Manchester and Salford Tar).

Benzene.	Toluene.	Phenol proper.	Solvent Naphtha for Indiarubber, containing the three Xylenes.	Heavy Naphtha.	Naphthalene.	Creosote.	Heavy Oil.	Anthracene.	Pitch.
lb. 1·10 = Aniline 1·10	lb. 0·90 = Toluidine 0·77	lb. 1·5	lb. 2·44 yielding 0·12 Xylene = 0·07 Xylidine	lb. 2·40	lb. 6·30 = α Naphthylamine 5·25 = α or β Naphthol 4·75 = Vermilline Scarlet, RRR 7·11 or = Naphthol Yellow * 9·50	lb. 17·0	lb. 14	lb. 0·46	lb. 69·6
= Magenta 0·623		Aurin 1·2						Alizarin 20 % 2·25.	
or 1·10 lb. Aniline yield 1·23 lb. Methyl Violet.									

DYEING POWER OF COLOURS FROM 1 TON OF LANCASHIRE COAL.

lb. 0·623 Magenta or dye 500 yards 27 in. wide Flannel a full shade.	lb. 1·23 Methyl Violet dye 1000 yards 27 in. wide Flannel a full Violet.	lb. 9·50 Naphthol Yellow or dye 3800 yards 27 in. wide Flannel a full Yellow.	lb. 7·11 Vermilline dye 2560 yards 27 in. wide Flannel a full Scarlet.	lb. 1·2 Aurin dye 120 yards 27 in. wide Flannel a full Orange.	lb. 2·25 Alizarin 20 % dye 255 yds Printer's cloth a full Turkey Red.

DYEING POWER OF COLOURS FROM 1 LB. OF LANCASHIRE COAL.

Magenta or **a piece of Flannel** **8 in. by 27 in.**	**Violet** **a piece of Flannel** **24 in. by 27 in.**	**Yellow** or **a piece of Flannel** **61 in. by 27 in.**	**Scarlet** **a piece of Flannel** **41 in. by 27 in.**	**Orange** **a piece of Flannel** **1·93 in. by 27 in.**	**Turkey Red** **a piece of Flannel** **4 in. by 27 in.**

*** The Naphthol Yellow is a representative colour from α Naphthol, while the Vermilline Scarlet is a representative colour from the combination of α Naphthylamine with β Naphthol.**

The several colours here chosen as examples are only a few amongst a very numerous list of varied colour derivatives of each group. Thus we are at present acquainted with about sixteen distinct yellow colours; about twelve orange; more than thirty red colours; about fifteen blues, seven greens, and nine violets; also a number of browns and blacks, not to speak of mixtures of these several chemical compounds, giving rise to an almost infinite number of shades and tones of colour. These colours are capable of a rough arrangement according as they are originally derived from one or other of the hydrocarbons contained in the coal-tar. The fifty specimens of different colours exhibited may thus be classified, but in this Table, for the sake of brevity, only the commercial names and not the chemical formulæ of these compounds is given.

Azo-colours.—Amongst the most important of the artificial colouring matters may be classed the so-called azo-colours. These colours are chiefly bright scarlets, oranges, reds, and yellows, with a few blues and violets. They owe their existence to the discovery by Griess, in 1860, of the fact that the so-called azo-group $- N = N -$ can replace hydrogen in phenols and amido compounds. But it is to Dr. O. N. Witt that is due the honour of having given the first start in a practical direction to the chrysoidine class of azo-colours by the discovery of chrysoidine, and perhaps still more so by the suggestions contained in a paper read before the Chemical Society. Dr. Caro, of Mannheim, was also acquainted with several compounds which belong to this class at the time Witt published his results, but it does not appear that he made practical use of them until Witt introduced the chrysoidines and tropeolines. To Roussin, of the firm of Poirrier of Paris, is due the credit of having first brought into the market some of the beautiful azo-derivatives of naphthol. Griess, therefore, as the original discoverer of the typical compounds and reactions by which the azo-colours are obtained, may be considered as the grandfather, whilst Roussin and Witt are really the fathers, of the azo-colour industry. Nor must it be forgotten that it is to Perkin we owe the recognition of the value of the sulpho group in relation to azo-colours, a discovery patented in 1863. Moreover it is interesting to note that changes in colour from yellow to red and claret are effected by the increase in the molecular weights of the radicals introduced as well as by the relative positions occupied by these groups.

Indophenol.—Witt is also the discoverer of a new blue dye-stuff termed indophenol, which has been used as a substitute for indigo. Certain difficulties, however, have arisen in the adoption of this colour on the large scale. The most important use indophenol is at present put to is for producing dark blues on reds dyed with azo-colours, both on wool and cotton. The piece goods are dyed a uniform red first, and then printed with indophenol white; for like indigo itself indophenol yields a colourless body on reduction, and this being a very powerful reducing agent destroys the azo-colour, being itself transformed into indophenol blue. The process works with surprising

TABLE II.

	1. BENZENE.	2. TOLUENE.	3. PHENOLS.	4. XYLENE.	5. NAPHTHALENE.	6. ANTHRACENE.
Yellows	Orange Yellow, or Acid Yellow.	..	Picric Acid	..	Manchester Yellow (Dinitronaphthol)	Alizarin (pure)
	Metanil Yellow	..	..	..	Naphthol Yellow	Anthrapurpurin
	Auramine	..	..	..	..	
Browns	Brown, Y	Bismarck Brown, R	..	..	..	Flavopurpurin
Oranges	Diphenylamine Orange (Blackley Orange)	..	..	..	..	
	Chrysoidine, Y	Chrysoidine, R	Aurin	..	..	
	Orange I. (mixture of 1 and 5)	..	..	..	..	
	Orange II. (1 and 5)	..	..	..	..	
	Orange III. (Helianthine)	..	..	..	..	
	Orange IV.	..	..	..	..	
Reds	Safranin	Magenta, R	Eosin	..	Bordeaux	
	..	Magenta, B	Safrosin	..	Vermilline Scarlet, R	
	..	..	Cyanosine	..	Vermilline Scarlet, R R R	
	..	..	Rose Bengal	..	Vermilline Scarlet, B B B	
	..	..	Phloxin	..	Roccellin (Mixture 1 and 5)	
	..	..	Erythrosin	(Mixture of Xylene and Naphthalene)	New Red	
	..	..	..	Xylidine Scarlet	Biebrich Scarlet	
	..	..	..	(Mixture of Cumene and Naphthalene)	Crocein Scarlet	
	..	..	..	Cumidine Scarlet	..	
Blues	Diphenylamine Blue	Blackley Blue, R	..	..	(Mixtures of 1 and 5) Victoria Blue, 1	
	Methylene Blue	Blackley Blue, 1	..	..	Victoria Blue, 5	
	Indulin (Campbelline)	Alkali Blue, R	..	..	..	
	..	Alkali Blue, 6 B	..	..	..	
Violets	Methyl Violet, 6 B	..	..	..	..	
	Methyl Violet, R	..	..	..	..	
Greens	Malachite Green	..	..	..	..	
	Brilliant Green	..	..	..	..	
	Acid Green (Acid Green)	..	..	..	..	

nicety and is very cheap. The blue is formed and the red discharged with such precision that patterns can be produced in which the blue discharge covers a great deal more space than the original red. This new printing process was devised by Mr. H. Koechlin, of Lorrach. The reds used for the purpose are in the case of wool, the usual azo-scarlets, for cotton congo-red.

Artificial Indigo.—About five years ago the speaker had the honour of bringing before this audience * the remarkable discovery made by Baeyer of the artificial production from coal-tar products of indigo blue. Since that time but little progress has been made in this manufacture, as the cost of the process, unlike the case of alizarin, has as yet proved too serious to enable the artificial to compete successfully in the market with the natural indigo.

Through the kindness of a number of eminent colour manufacturers in this country and on the Continent, the speaker was enabled to illustrate his subject by a most complete series of specimens both of the colours themselves and of their application to the dyeing and printing of fabrics of all kinds. His thanks are especially due to his friend, Mr. Ivan Levinstein, of Manchester, for the interesting series of samples of cloth dyed with known quantities of fifty different coal-tar colours, each having a different chemical composition; also to the same gentleman, and to Messrs. Burt, Boulton, and Haywood, of London, for the interesting and unique series of specimens indicating the absolute quantities of products obtainable from *one ton of coal*, as well as for much assistance on the part of Mr. Levinstein in the preparation of the experimental illustrations for this discourse. To Dr. Martius of Berlin for a valuable series of colours, especially the well-known Congo red, made by his firm, including samples of wool dyed therewith, he is also much indebted. For the interesting details concerning indophenol and its applications the speaker owes his thanks to Dr. Witt and M. Koechlin.

Coal-tar Antipyretic Medicines.—Next in importance to the colour industry comes the still more novel discovery of the synthetical production of antipyretic medicines.

Up to this time quinine has held undisputed sway as a febrifuge and antiperiodic, but the artificial production of this substance has as yet eluded the grasp of the chemist. Three coal-tar products have, however, been recently prepared which have been found to possess strong febrifuge qualities, which if still in some respects inferior to the natural alkaloids, yet possess most valuable qualities, and are now manufactured in Germany at Höchst and at Ludwigshafen in large quantity. And here it is well to call to mind that the first tar colouring-matter discovered by Perkin (mauve) was obtained in 1856 during the prosecution of a research which had for its object the artificial production of quinine.

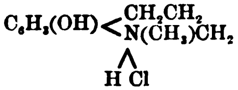
---

* 'On Indigo and its Artificial Production,' Proc. Roy. Inst., May 27th, 1881.

In considering the historical development of this portion of his subject, the speaker added that it is interesting to remember that the initiative in the production of artificial febrifuges was given by Professor Dewar's discovery in 1881 that quinoline, the basis of these antipyretic medicines, is an aromatic compound, as from it he obtained aniline. Moreover, that Dewar and McKendrick were the first to observe that certain pyridine salts act as febrifuges. So that these gentlemen may be said to be the fathers of the antipyretic medicines, as Witt and Roussin are of the azo-colour industry.

Kairine, the first of these, was discovered by Prof. O. Fischer, of Munich, in the year 1881, whilst engaged on his investigations of the oxyquinolines. The febrifuge properties of this substance were first noticed by Prof. Filehne, of Erlangen. Kairine is manufactured from quinoline, a basic product derived from aniline by heating it with glycerin and nitrobenzene by the following process. When treated with sulphuric acid, SO_4H_2, it forms quinoline sulphonic acid, and this when fused with caustic soda yields *oxyquinoline*, which is then reduced by tin and hydrochloric acid into tetrahydroxyquinoline, and this again on treatment with C_2H_5Br yields ethyl-tetraoxyquinoline or kairine. The lowering of the temperature of the body by this compound is most remarkable, though, unfortunately, the action is of much shorter duration than that effected by quinine itself; but on the other hand, with the exception of its burning taste, it exerts no evil effects such as are often observed after administration of large doses of quinine. The commercial article is the hydrochloride, the price is 85*s*. per lb., and the quantity manufactured has lately diminished owing to the discovery of the second artificial febrifuge, antipyrine.

The following graphical formula shows the constitution of kairine:—



$$C_6H_3(OH)<\begin{array}{l}CH_2CH_2\\ \underset{\overset{\wedge}{H\,Cl}}{N}(CH_3)CH_2\end{array}$$

Antipyrine, the second of these febrifuges, was discovered in 1883 by Dr. L. Knorr in Erlangen, and its physiological properties were investigated by Prof. Filehne of Erlangen. The materials used in the manufacture of antipyrine are aniline and aceto-acetic ether. The aniline is first converted into phenylhydrazine, a body discovered by Emil Fischer in 1876. This body combines directly with aceto-acetic ether, with separation of water and alcohol, to form a body called pyrazol ($C_{10}H_{10}N_2O$). The methyl derivative of pyrazol derived by treating it with iodide of methyl, is *antipyrine*, its composition being $C_{11}H_{12}N_2O$. As a febrifuge, antipyrine is superior in many respects to kairine and even to quinine itself. It equals kairine in the certainty of its action whilst in its duration it resembles quinine. It is almost tasteless and odourless, is easily soluble in cold water, and takes the form of a white crystalline powder. Its use as a medicine is accompanied by no drawbacks. It occurs in commerce in the free state. The production of antipyrine, in spite of these valuable qualities, is as yet small, its chief employment being in Germany, where it has been successfully used in cases of typhoid epidemic. The price is 6*s*. per pound.

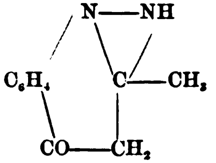
The following equations explain the formation and constitution of this interesting body. The foregoing febrifuges are manufactured at Höchst under the superintendence of Dr. Pauli, to whose kindness the speaker is indebted for an interesting series of specimens illustrative of the manufacture of antipyrine.

$$\underset{\text{Acetoacetic ether}}{CH_3.CO.CH_2.CO_2C_2H_5} + \underset{\text{Phenylhydrazine}}{C_6H_5.NH.NH_2}$$

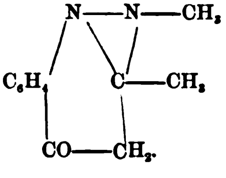
$$= H_2O + C_2H_5.OH + \underset{\text{Pyrazol}}{C_{10}H_{10}N_2O}$$

$$C_{10}H_{10}N_2O + ICH_3 = \underset{\text{Antipyrine-hydriodide}}{IH.C_{10}H_9(CH_3)N_2O}$$

Dr. Knorr formulates pyrazol thus:



And antipyrine is



The antipyretic effect of this compound is strikingly shown in the following temperature readings in a case of typhoid kindly communicated to the speaker by his friend Dr. Dreschfeld of Manchester. Each of the second set of readings was made two hours after a dose of 30 grains of antipyrine had been administered.

I.	II.	Diff.
105°·0	103°·0	2°·0
103·5	100·2	3·2
103·8	100·8	3·0
105·2	101·4	3·8
104·4	100·6	3·8

Thalline.—The third of the artificial febrifuges is *thalline*, which is offered as the tartrate and sulphate. It is manufactured by the Badische Company. Thalline is said to be used as an antidote for yellow fever. Its scientific name is tetrahydroparaquinanisol, and it was first prepared by Skraup by the action of methyl iodide and potash on paroxyquinoline.

We must, however, bear in mind that none of these synthetical febrifuges are antiperiodics, and therefore cannot be employed instead of the natural alkaloid quinine in cases of ague or intermittent fevers.

Coal-tar Aromatic Perfumes.—A third group of no less interest comprises the artificial aromatic essences, and of these may here be mentioned, in the first place, *cumarin*, $C_9H_6O_2$, the crystalline solid found in the sweet woodruff, in Tonka bean, and in certain sweet-scented grasses. This is now artificially prepared by acting upon sodium salicyl aldehyde with acetic anhydride by the reaction which is associated with the name of Dr. Perkin, and is used in the manufacture of the perfume known as "extract of new-mown hay."

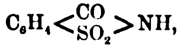
A second interesting case of a production of a naturally occurring flavour, is the artificial production of *vanillin*, the crystalline principal of vanilla. Vanilla is the stalk of the *Vanilla planifolia*, which incloses in its tissues prisms of crystalline vanillin, to which substance it owes its fragrance. Tiemann and Harrmann showed that vanillin is the aldehyde of methyl protocatechuic acid

$$C_6H_3(OH)(OCH_3)CHO, [CHO : OCH_3 : OH = 1 : 3 : 4].$$

The chief seats of the vanilla productions are on the slopes of the Cordilleras north-west of Vera Cruz in Mexico, also the island of Réunion, and in the Mauritius. Since the discovery of the artificial production of vanillin, the growth of the vanilla has been very much restricted.

A variety of vanilla, termed vanillon, obtained in the East Indies, has long been used in perfumery for preparing "essence of heliotrope." This contains vanillin together with an oil, which is probably oil of bitter almonds. The essence of white heliotrope is now entirely prepared by synthetical operations. It is manufactured by adding a small quantity of artificial oil of bitter almonds to a solution of artificial vanillin; when these substances are allowed to remain for some time in contact, the mixture assumes an odour closely resembling that of natural heliotrope. Through the kindness of Mr. Rimmel the speaker was able to render the fragrance of this coal-tar perfume perceptible to his audience. Nor must we forget to mention the so-called essence of mirbane (nitrobenzene), of which about 150 tons per annum are used for perfuming soap; and artificial oil of bitter almonds, employed as a flavour in place of the natural oil.

Coal-tar Saccharine.—Of all the marvellous products of the coal-tar industry, the most remarkable is perhaps the production of a sweet principle surpassing sugar in its sweetness *two hundred and twenty* times. This substance is not a sugar, it contains carbon, hydrogen, sulphur, oxygen, and nitrogen. Its formula is



$$C_6H_4 < {CO \atop SO_2} > NH,$$

and its chemical name is benzoyl sulphonic imide, or for common use, saccharine. It does not act as a nutriment, but is non-poisonous, and passes out of the body unchanged. The following is a concise statement of its properties, and mode of production from the toluene of coal-tar. It should, however, be first mentioned that the compound benzoyl sulphonic imide (saccharine) was first discovered by Constantin Fahlberg and Remsen, in America. But no patent was taken out for a commercial process till recently, and it is now patented in this country.

Step I.—Toluene is treated with fuming sulphuric acid in the cold, or it is heated with ordinary sulphuric acid of 168½° Twaddell on the water-bath, or not above 100° C. The latter method is the better. The acid is best caused to act upon the toluene in closed vessels rotating on horizontal axles.

$$\underset{\text{Toluene.}}{C_6H_5CH_3} + SO_4H_2 = \underset{\substack{\text{Toluene sulphonic acids}\\\text{(ortho and para).}}}{C_6H_4\left\{\begin{matrix}CH_3\\SO_2.OH\end{matrix}\right.} + H_2O.$$

Step II.—After all toluene (which as toluene is insoluble in the acid) has disappeared, the contents of the agitating vessel are run into wooden tanks in part filled with cold water, and the whole liquid is stirred up with chalk to neutralise the excess of sulphuric acid used and to obtain the two isomeric toluene sulphonic acids as calcium salts.

$$2\underset{\substack{\text{Toluene, ortho- and}\\\text{para-sulphonic acids}}}{\left(C_6H_4\left\{\begin{matrix}CH_3\\SO_2.OH\end{matrix}\right.\right)} + SO_4H_2 + 2(CaCO_3) =$$

$$\underset{\substack{\text{Calcium toluene ortho-}\\\text{and para-sulphonates}}}{\left(C_6H_4\left\{\begin{matrix}CH_3\\SO_3\end{matrix}\right.\right)_2Ca} + CaSO_4 + 2CO_2 + 2H_2O.$$

The neutralised mass is filtered through a filter-press to separate therefrom the precipitate of gypsum, which is washed with hot water, and the washings added to the filtrate.

Step III.—The calcium salts are now treated with carbonate of sodium, to obtain the sodium salts, with precipitation of carbonate of calcium. The precipitate is removed by means of a filter-press from the solution containing the sodium ortho- and para-sulphonates.

$$\left(C_6H_4\left\{\begin{matrix}CH_3\\SO_3\end{matrix}\right.\right)_2Ca + Na_2CO_3 = CaCO_3 + \underset{\text{The sodium toluene sulphonates}}{2C_6H_4\left\{\begin{matrix}CH_3\\SO_2.ONa.\end{matrix}\right.}$$

Step IV.—The solution of the sodium salts from III. is evaporated either in an open- or in a vacuum-pan so far that a portion taken out will solidify on cooling. The contents of the pan are then run into moulds of wood or iron, and allowed to cool and solidify. The lumps are at length taken from the moulds, broken up small, and dried in a drying-room, and subsequently in a drying apparatus heated with steam, until quite desiccated.

Step V.—The sodium sulphonate salts are now converted into their corresponding sulphonic chlorides. This is effected as follows:—The dried sulphonates are thoroughly mixed with phosphorus trichloride, itself as dry as possible. The mixture is then placed in lead-lined iron vessels, and a current of chlorine is passed over the mixture till the reaction is ended. The temperature generated by the reaction must be properly regulated by cooling the apparatus with water. The phosphorus oxychloride resulting from the decomposition is driven off, collected, and utilised for developing chlorine from bleaching powder for the chlorinating process, phosphate of lime being precipitated, which can be used in manures. For this purpose the oxychloride is treated with water, and the mixture, now containing hydrochloric and phosphoric acids, is brought into contact with the chloride of lime.

The reaction by which the ortho- and para-toluene sulphonic chlorides are produced is indicated by the following equation:—

$$C_6H_4\left\{\begin{matrix}CH_3\\SO_2Na\end{matrix}\right. + (PCl_3 + 2Cl) = \underset{\text{Toluene sulphonic chlorides}}{C_6H_4\left\{\begin{matrix}CH_3\\SO_2Cl\end{matrix}\right.} + POCl_3 + NaCl$$

The two sulphonic chlorides remaining in the apparatus are allowed to cool slowly, when the solid one (the para compound) is deposited in large crystals, so that the liquid one can be easily removed by the aid of a centrifugal machine. The crystalline residue is freed from all the liquid sulphonic chloride by washing with cold water. Only the liquid orthotoluene sulphonic chloride is capable of yielding saccharine, and the liquid product above separated is cooled with ice to crystallise out the last traces of the crystalline compound. The solid parasulphonic chloride obtained as by-product, is decomposed into toluene, hydrochloric, and sulphurous acids by mixing it with carbon, moistening the mixture, and subjecting it under pressure to the action of superheated steam. The total change proceeds in two stages:—

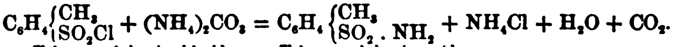
$$1.\ C_6H_4\left\{\begin{matrix}CH_3\\SO_2Cl\end{matrix}\right. + H_2O = C_6H_4\left\{\begin{matrix}CH_3\\SO_2OH\end{matrix}\right. + HCl.$$

$$2.\ 2\left(C_6H_4\left\{\begin{matrix}CH_3\\SO_2OH\end{matrix}\right.\right) + C = 2\,(C_6H_5.CH_3) + CO_2 + SO_2.$$

The toluene is then used again in Step I., and the hydrochloric and sulphurous acids in Step VII.

Step VI.—The liquid orthotoluene sulphonic chloride is now converted into the orthotoluene sulphonic amide by treating the former with solid ammonium carbonate in the required proportions, and subjecting the resulting thick pulpy mixture to the action of steam.

Carbonic acid is set free, and a mixture of orthotoluene sulphonic amide and ammonium chloride remains.

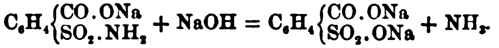


$$C_6H_4\left\{\begin{matrix}CH_3\\SO_2Cl\end{matrix}\right. + (NH_4)_2CO_3 = C_6H_4\left\{\begin{matrix}CH_3\\SO_2\,.\,NH_2\end{matrix}\right. + NH_4Cl + H_2O + CO_2.$$

Toluene sulphonic chloride Toluene sulphonic amide

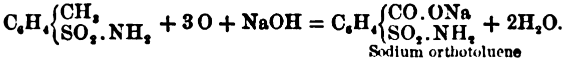
As the mixture is very liable to solidify on cooling, cold water is at once added to prevent this, and to dissolve out the ammonium chloride, the amide remaining in the solid state. The liquid is separated by centrifugating.

STEP VII.—The orthotoluene sulphonic amide is now oxidised, preferably by means of potassium permanganate. The result of this will be, precipitated manganese dioxide, free alkali and alkaline carbonate, and an alkaline orthosulphamido-benzoate. The alkaline liquid requires careful neutralisation during the oxidising process, and especially before evaporating, with a mineral acid, or else the sulphamido-benzoate formed would be again split up into orthosulphonic benzoate and free ammonia, thus:—



$$C_6H_4\left\{\begin{matrix}CO.ONa\\SO_2.NH_2\end{matrix}\right. + NaOH = C_6H_4\left\{\begin{matrix}CO.ONa\\SO_2.ONa\end{matrix}\right. + NH_3.$$

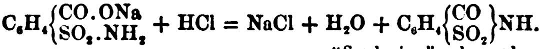
The oxidation process itself is thus represented:—



$$C_6H_4\left\{\begin{matrix}CH_3\\SO_2.NH_2\end{matrix}\right. + 3\,O + NaOH = C_6H_4\left\{\begin{matrix}CO.ONa\\SO_2.NH_2\end{matrix}\right. + 2H_2O.$$

Sodium orthotoluene sulphamido-benzoate

By precipitation with dilute mineral acids, such as hydrochloric or sulphurous acids, the pure benzoyl sulphonic imide is at once precipitated:—



$$C_6H_4\left\{\begin{matrix}CO.ONa\\SO_2.NH_2\end{matrix}\right. + HCl = NaCl + H_2O + C_6H_4\left\{\begin{matrix}CO\\SO_2\end{matrix}\right\}NH.$$

"Saccharine," or benzoyl sulphonic imide

Saccharine possesses a far sweeter taste than cane sugar, and has a faint and delicate flavour of bitter almonds. It is said to be 220 times sweeter than cane sugar, and to possess considerable antiseptic properties. On this account, and because of its great sweetness, it is possible that it may be useful in producing fruit preserves or jams, consisting of almost the pure fruit alone; the small percentage of saccharine necessary for sweetening these preserves being probably sufficient to prevent mouldiness. Saccharine has been proved by Stutzer, of Bonn, to be quite uninjurious when administered in considerable doses to dogs, the equivalent as regards sweetness in sugar administered, being comparable to over a pound of sugar each day. Stutzer found, moreover, that saccharine does not nourish as sugar does, but that it passes off in the urine unchanged. It is proposed thus to use it for many medical purposes, where cane sugar is excluded from the diet of certain patients, as in cases of "diabetes mellitus," and in this respect it may prove a great boon to suffering humanity, although we must remember that, as certain of the aromatic compounds if administered for a length of time are known to exert a physiological effect, especially on the liver, it will be desirable to use caution in the regular use of saccharine until its harmless action on the human body has been ascertained beyond doubt.

Saccharine is with difficulty soluble in cold water, from hot aqueous solutions it is easily crystallised. Alcohol and ether easily dissolve it. Hence from a mixture of sugar and saccharine, ether would easily separate the saccharine by solution, leaving the sugar. It melts at about 200° C. with partial decomposition.

The taste is a very pure sweet one, and in comparison with cane sugar it may be said that the sensation of sweetness is much more rapidly communicated to the palate, on contact with saccharine, than on contact with sugar. The speaker expressed his thanks to the discoverer of saccharine, Dr. Fahlberg, of Leipzic, for a complete and interesting series of preparations illustrating the domestic and medicinal uses of this remarkable compound, and also to his friend Mr. Watson Smith for the kind aid afforded him in the experimental illustration of his discourse.

[H. E. R.]

ANNUAL MEETING,

Saturday, May 1, 1886.

Sir Frederick Bramwell, F.R.S. Honorary Secretary and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1885, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to above 85,000*l.* entirely derived from the Contributions and Donations of the Members.

Twenty-six new Members paid their Admission Fees in 1885.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1885.

The Books and Pamphlets presented in 1885 amounted to about 354 volumes, making, with 464 volumes (including Periodicals bound) purchased by the Managers, a total of 818 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, K.G. D.C.L. LL.D.
Treasurer—Henry Pollock, Esq.
Secretary—Sir Frederick Bramwell, F.R.S.

Managers.

Sir Frederick Abel, C.B. D.C.L. F.R.S.
Sir William Bowman, Bart. LL.D. F.R.S.
Joseph Brown, Esq. Q.C.
Sir James Crichton Browne, M.D. LL.D. F.R.S.
William Crookes, Esq. F.R.S.
Henry Doulton, Esq.
Sir William Withey Gull, Bart. M.D. D.C.L. F.R.S.
Right Hon. The Lord Halsbury.
William Huggins, Esq. D.C.L. LL.D. F.R.S.
Alfred Bray Kempe, Esq. M.A. F.R.S.
Sir John Lubbock, Bart. M.P. D.C.L. LL.D. F.R.S.
Hugo W. Müller, Esq. Ph.D. F.R.S.
Sir Frederick Pollock, Bart. M.A.
John Rae, M.D. LL.D. F.R.S.
Lord Arthur Russell.

Visitors.

Shelford Bidwell, Esq. M.A.
Stephen Busk, Esq.
Michael Carteighe, Esq. F.C.S.
Arthur Herbert Church, Esq. M.A. F.C.S.
Vicat Cole, Esq. R.A.
William Henry Domville, Esq.
James Edmunds, M.D. F.C.S.
Charles Hawksley, Esq. M.I.C.E.
Alfred Gutteres Henriques, Esq. F.G.S.
David Edward Hughes, Esq. F.R.S.
George Matthey, Esq. F.R.S.
John W. Miers, Esq.
Lachlan Mackintosh Rate, Esq. M.A.
William Chandler Roberts-Austen, Esq. F.R.S.
Alexander Siemens, Esq.

GENERAL MONTHLY MEETING,

Monday, May 3, 1886.

William Huggins, Esq. D.C.L. LL.D. F.R.S. Manager, in the Chair.

William Henry Allen, Esq.
Mrs. George Baden Crawley,
William James Farrer, Esq.
Alexander Gray, Esq.
John Rudd Leeson, M.D. F.G.S.
George John Romanes, Esq. M.A. LL.D. F.R.S.
Arthur William Rücker, Esq. M.A. F.R.S.
Lewis Richard Shorter, Esq.

were elected Members of the Royal Institution.

John Tyndall, Esq. D.C.L. LL.D. F.R.S. was re-elected Professor of Natural Philosophy.

The Managers reported that they received the following letter from Mr. **George Busk**, the late Treasurer of the Royal Institution :—

"32, Harley Street,
"*February 24th*, 1886.

"Dear Sir Frederick,

"I much regret that I shall not be able to attend the Committee this "afternoon, and as I fear from the state of my health that I shall very probably "not be equal to attending any meetings at the Royal Institution in future, I "should be obliged if you would be good enough to lay my resignation of the "Office of Treasurer, with which I have been honoured for so many years, before "the meeting of Managers on Monday next. I have further to beg that you will "assure the Managers that I do this with the greatest regret and from the fullest "conviction that it is for the best interests of the Royal Institution.

"Believe me, yours truly,
"Geo. Busk.

"Sir Frederick Bramwell, F.R.S."

The Managers further reported that at their meeting on April 5th last it was Resolved, "That Mr. Busk be informed that the Committee of Managers desire to express their very sincere regret that the state of his health should have rendered it necessary for him to resign an office which he has filled for so many years with advantage to the Institution and at a considerable sacrifice of time and labour to himself, and that they cannot accept his resignation without expressing their sense of the value of his past services and support to the Institution, and their very sincere hope that he may experience improved health in the future."

Resolved, "That this Meeting cordially adopt the Resolution of the Managers passed at their Meeting on the 5th of April in relation to the resignation of Mr. GEORGE BUSK from the office of Treasurer of the Royal Institution."

The Special Thanks of the Members were returned for the following donation to the Fund for the Promotion of Experimental Research:—

LACHLAN MACKINTOSH RATE, Esq. £50.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India. Palæontologia Indica; Memoirs, Series XIII. Vol. I. Part 5. fol. 1886.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quarta, Vol. VI. Nos. 1, 2. 8vo. And Disegni. fol. 1886.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. Vol. II. Fasc. 7. 8vo. 1886.
Asiatic Society of Bengal—Journal, Vol. LIV. Part 1, Nos. 3, 4; Part 2, No. 3. 8vo. 1885.
Proceedings, 1885, Nos. 9, 10. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. XLVI. No. 5. 8vo. 1886.
Bankers, Institute of—Journal, Vol. VII. Part 4. 8vo. 1886.
Bavarian Academy of Sciences—Abhandlungen, Band XV. 2te Abtheilung. 4to. 1885.
Sitzungsberichte, 1884, Heft 4, 1885. 8vo.
British Architects, Royal Institute of—Proceedings, 1885–6, Nos. 13, 14. 4to.
Dax: Société de Borda—Bulletin, Onzième Année. 1e Trimestre. 8vo. 1886.
Chemical Society—Journal for April, 1886. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. VI. Part 2. 8vo. 1886.
Editors—American Journal of Science for April, 1886. 8vo.
Analyst for April, 1886. 8vo.
Athenæum for April, 1886. 4to.
Chemical News for April, 1886. 4to.
Engineer for April, 1886. fol.
Horological Journal for April, 1886. 8vo.
Iron for April, 1886. 4to.
Nature for April, 1885. 4to.
Revue Scientifique for April, 1886.
Telegraphic Journal for April, 1886. 8vo.
Franklin Institute—Journal, No. 724. 8vo. 1886.
Geological Institute, Imperial, Vienna—Jahrbuch: Band XXXVI. Heft 1. 8vo. 1886.
Horticultural Society, Royal—Journal, Vols. V. VI. VII. No. 1. 8vo. 1877–86.
Johns Hopkins University—Studies in Historical and Political Science, Fourth Series, No. 4. 8vo. 1886.
American Chemical Journal, Vol. VIII. No. 1. 8vo. 1886.
Lettsom, W. G. Esq. M.R.I.—Elements of Electricity and Electro-Chemistry. By G. J. Singer. 8vo. 1814.
Linnean Society—Journal, Nos. 113, 142. 8vo. 1886.
Mechanical Engineers' Institution—Proceedings, 1886, No. 1. 8vo.
Medical and Chirurgical Society, Royal—Proceedings, No. 12. 8vo. 1886.
Catalogue of Library, 1885, Supplement IV. 8vo. 1886.

Meteorological Office—Meteorological Observations at Stations of the Second Order for 1881. 4to. 1886.

Miller, W. J. C. Esq. (the Registrar)—The Medical Register, 1886. 8vo.

The Dentists' Register, 1886. 8vo.

North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXV. Part 2. 8vo. 1886.

Odontological Society of Great Britain—Transactions, Vol. XVIII. No. 6. New Series. 8vo. 1886.

Perry, Rev. S. J. F.R.S. and Prof. B. Stewart, F.R.S. (the Authors)—Fluctuations of Declination at Kew and Stonyhurst, 1883–4. (Proc. Royal Society, 1885.)

Pharmaceutical Society of Great Britain—Journal, April, 1886. 8vo.

Photographic Society—Journal, New Series, Vol. X. No. 6. 8vo. 1886.

Physical Society—Proceedings, Vol. VII. Part 4. 8vo. 1886.

Royal Society of London—Proceedings, No. 242. 8vo. 1886.

Saxon Society of Sciences, Royal—Philologisch-Historische Classe: Berichte, 1885. No. 4. 8vo. 1886.

Second Geological Survey of Pennsylvania—Grand Atlas: Div. I. County Geological Maps, Part 1; Div. II. Anthracite Coal Fields, Parts 1 and 2; Div. III. Petroleum and Bituminous Coal Fields, Part 1; Div. IV. South Mountain and Great Valley Topographical Maps; Div. V. Central and S.E. Pennsylvania, Part 1. 1884–5.

Snell, H. Saxon, Esq. M.R.I. (the Author)—Circular Hospital Wards. (Trans. Sanitary Inst.) 1885.

Hull Royal Infirmary. 8vo. 1885.

St. Marylebone Infirmary. 8vo. 1886.

Society of Arts—Journal, April, 1886. 8vo.

St. Pétersbourg, Académie des Sciences—Mémoires, Tome XXXIII. No. 5. 4to. 1886.

Statistical Society—Journal, Vol. XLIX. Part 1. 8vo. 1886.

Surgeon-General's Office, U.S. Army—Index-Catalogue of the Library, Vols. 1–6. 4to. 1880–5.

Telegraph Engineers, Society of—Journal, No. 60. 8vo. 1886.

United Service Institution, Royal—Journal, No. 133. 8vo. 1886.

University of London—Calendar, 1886–7. 8vo.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1886: Heft 3. 4to.

Victoria Institute—Journal, No. 76. 8vo. 1886.

Wise, Thomas A. M.D. (the Author)—History of Paganism in Caledonia. 4to. 1884.

Zoological Society—Transactions, Vol. XII. Part 2. 4to. 1886.

Proceedings, 1885, Part 4. 8vo.

WEEKLY EVENING MEETING,

Friday, May 7, 1886.

WILLIAM HUGGINS, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

FREDERICK SIEMENS, Esq.

On Dissociation Temperatures with special reference to Pyrotechnical questions.

IN bringing the subject of dissociation before the Royal Institution of Great Britain, I wish it to be understood that I propose to confine myself to its influence on combustion and heating, that is to say, to its effects on combustible gases and the products of combustion, and on furnace work generally. My researches have been made for the most part in connection with large gas furnaces constructed according to my new system of working with radiated heat, or what may be otherwise called free development of flame. In perfecting this system of furnace the principle of which is in many respects the reverse of that generally accepted, both as regards construction and working, I had to examine into the accuracy of certain scientific theories which could not be brought into harmony with the actual results I obtained.

In order that I may be clearly understood it is necessary to describe shortly my system of furnaces before entering upon the theory which alone appears to explain satisfactorily the practical results obtained by its means. These furnaces have of late been largely introduced and are now extensively applied. I first described them in a paper read at the Meeting of the Iron and Steel Institute held at Chester in September 1884; their main peculiarity consists in the arrangements by which the heat is abstracted in two different ways, and at two different periods. In the first, or active, stage of combustion the flame passes through a large combustion chamber (all contact with its surfaces being avoided), and parts with its heat by radiation only; while in its second stage the products of combustion are brought into direct contact with the surfaces and materials to be heated, by which means the remainder of its heat is abstracted. This, in a few words, is a description of the method of heating with free development of flame, and it now only remains to explain how to construct fireplaces and furnaces on this principle.

As regards its principal application hitherto, namely, to regenerative gas furnaces, the two successive stages of heating are, by radiation in the furnace chamber, and by contact in the regenerators. The flame during active combustion heats the furnace chamber and material placed therein by radiation only, and as soon as this stage is completed the fully burnt gases enter the regenerative chambers and deposit their remaining heat by coming into contact with the loose brickwork filling them. As it is essential that the flame during its first stage, while still in chemical action, should give up heat by radiation only, it is found absolutely necessary that it should not touch the sides or walls of the furnace chamber, or any material contained in the furnace. The sides and arches of the furnace and the flues leading into it must therefore be so arranged that *the flame does not touch anything*, and its length must be sufficient to allow time for complete combustion before the flame leaves it. When regenerative furnaces are arranged so as to fulfil these conditions the heat developed by the flame is much more intense than otherwise, while notwithstanding the higher temperature and increased working power attained, their durability is largely augmented.

Intensity of temperature and durability are two advantages of the greatest importance which were formerly seldom found combined in furnaces. The manner in which these advantages are insured in the radiation furnace may be thus explained :—

Adopting the generally accepted theory of combustion, according to which a flame consists of a chemically excited mixture of gases, whose particles are in violent motion, either oscillating to and from each other or rotating around one another, it follows that any solid substance brought into contact with gases, thus agitated, must necessarily have an impeding effect on their motion. Motion being the primary condition of combustion, the latter will be more or less interfered with, according to the greater or less extent of the surfaces which impede the action of the particles forming the flame; in the immediate neighbourhood of such surfaces the combustion of the gases will cease altogether, because the attractive influence of the surfaces will entirely prevent their motion; further off, their combustion will be partial, and only at a comparatively great distance the particles of gas will be free to continue unimpeded the motion required to maintain combustion. On the other hand, the surfaces themselves must suffer from the motion of the particles of gas producing the flame, for however small these particles may be, they produce, while in such violent motion, an amount of energy which acting constantly will in time destroy the surfaces opposed to them, just as "continual dropping wears away stone." This circumstance fully accounts for the fact that the inner sides of furnaces, and the materials they contain, are soon destroyed, not by heat, but by the mechanical, and perhaps also by the chemical action of the flame. It would seem strange that the heating power of a large volume of flame should be so much interfered with by the contact of its outer parts only with the inner sides of a large furnace chamber, if there was not another cause besides imperfect combustion to reduce the heating effect of a flame, which touches the surfaces to be heated. A flame when in the state of combustion radiates heat not only from its outer surface, but also from its interior by allowing the heat to radiate through its mass. In this manner every particle of flame sends its rays in all directions, but if the flame itself touches anywhere combustion ceases there, free carbon is liberated and produces smoke which envelopes that part and prevents the rays of heat of the other portions of the flame from reaching it.

Radiation plays a much greater part in all heating operations than has been hitherto acknowledged, consequently any cause which tends to lessen the radiating power of flame, or to screen its rays, reduces also the amount of heat which can be thus utilized.

If the flame is not allowed to come into contact with bodies to be heated, combustion is improved, while full advantage is also gained of its heat-radiating power, which would otherwise be diminished more or less, as already explained. The ordinary mode of applying flame, by allowing it to impinge directly upon the surfaces to be heated, causes imperfect combustion, prevents the rays of heat from reaching them, and also destroys, or tends to destroy them; this is particularly the case when hydrocarbons and carbonic oxide are used. These statements are fully borne out by the results which I have obtained in practice with the new and old form of regenerative furnace respectively, and they also fully agree with the theoretical explanation I have suggested; that theory, however, is still incomplete, as it does not deal with the subject of dissociation, a subject to which for various reasons I have avoided referring until recently, although it has been brought forward by several writers, and used as an argument against my new system of furnace; as according to these writers it would appear to be impossible to produce such exceedingly high temperatures as I claim to reach. I have long held the opinion that appearances of dissociation not being observable in furnaces heated by radiation, but occurring in furnaces in which the flame is allowed to come into contact with surfaces, must be due to the action on the flame of those surfaces at high temperature. I was led to this conclusion partly from my own observations, and partly from descriptions of dissociation observed by others, amongst whom was my brother, the late Sir William Siemens, who described a case of dissociation (see lecture delivered March 3rd, 1879, at the Royal United Service Institution, entitled "On the production of Steel and its application to military purposes") which occurred in a regenerative gas furnace constructed according to our old views of combustion and heating. *The conclusion at which I have arrived is, that solid surfaces, besides obstructing active combustion, must also at high temperatures have a dissociating influence on combustible gases and on the products of combustion.*

In order to obtain information on this subject I examined the laws and theory of dissociation, and endeavoured to bring the various results obtained by scientific authorities into agreement with one another, and with my own experience, but failed entirely in doing so. The temperatures of dissociation of carbonic acid and steam, the two principal gases forming the products of combustion when ordinary fuel is used, vary very much according to these observers, and the results I have obtained in practice are different from most of them. I hope to prove that the temperature at which dissociation sets in, is, in most cases, much higher than generally admitted; and that the authorities I am about to refer to have omitted in almost all the experiments they have made to take into proper consideration one element which is liable to alter materially the results obtained by them. *This element is the surface, form, and material of the apparatus used for those experiments.*

In considering the question of dissociation, I propose to commence with Deville, who first discovered and called attention to the dissociation of gases at high temperatures. He made numerous experiments with various gases, dissociating steam, carbonic acid, and also carbonic oxide (in the latter case producing carbonic acid and carbon), and fixed certain temperatures at which he found that either complete or partial dissociation took place. Without going into details I may mention that Deville required to use vessels and tubes of definite dimensions, material, and structure, in order to obtain the results stated. One experiment had to be made with a porous tube, another required the use of a vessel with rough interior surfaces, or containing some rough or smooth material. In this way Deville arrived at a great variety of results, and although he does not state that the rough surfaces, or porous tubes, or the solid material placed inside the vessels which he employed, had any particular influence on the temperature at which dissociation took place, yet it would appear that he could not obtain his results without having recourse to those means. Deville's results depended very much upon the various kinds of surfaces he used in his experiments, if they were not entirely brought about by them; these experiments, moreover, were of a very complicated nature, so I propose to pass on to more modern authorities whose experiments are of simpler character, and less open to objection.

The most important experiments, which modify those of Deville, are due to Bunsen. Bunsen observed the dissociation of steam and carbonic acid by employing small tubes filled with an explosive mixture of these gases, to which suitable pressure gauges were attached. On igniting the gaseous mixture explosion took place, and a high momentary pressure was produced within the tube; from the pressure developed Bunsen calculated the temperature at which the explosion took place, and found that it varied with the mixtures employed. He records the circumstance that only about one-third of the combustible gases took part in the explosion, from which circumstance he concluded that the temperature attained was the limit at which combustion occurred. To prove this, Bunsen allowed the gases sufficient time to cool, after which a second explosion was produced, and even a third explosion when time was allowed for the gases to cool down again. Bunsen's theory seems very plausible, besides which he obtains much higher temperatures for his limits of dissociation than other physicists, so that I might have accepted the figures at which he arrives; these are for steam about 2400° C., and for carbonic acid about 3000° C. These temperatures are probably higher

than are reached in the arts, as materials used in furnace-building would not withstand such temperatures for any length of time; but still I must call attention to the circumstance that if the influence of the inner surfaces of the tubes on the combustion of the gases therein could be removed, the dissociation temperatures arrived at would be found still higher. I cannot admit that Bunsen's explanation of the cause of the second and third explosions is quite satisfactory, as it is not the cooling of the gases alone which renders the subsequent explosions possible, but also the thorough re-mixture of the gases by diffusion after each explosion. This I will illustrate by means of the diagrams exhibited, Figs. 1 to 6, which represent:—

1. A tube filled with an explosive gas mixture which is shown white.
2. The same tube immediately after an explosion has taken place, the white margin indicating the unexploded mixture close to the sides, and the deep-red, towards the middle of the tube, the exploded gases. The white is shown as merging into deep-red by degrees, because close up to the sides the surfaces prevent explosion or combustion altogether; nearer the middle partial combustion takes place, whilst only in the middle of the tube the gases find sufficient space for complete combination.
3. The same tube after the burnt and unburnt gases have mixed by means of diffusion, which is coloured light-red.
4. The same tube immediately after the second explosion, coloured light-red at the sides, turning into deep-red by degrees towards the middle.
5. The same tube after diffusion has done its work a second time, coloured a deeper shade of red.
6. The same tube after the third explosion, coloured nearly deep-red throughout, but still a lighter shade on the sides.

In Bunsen's mode of determining dissociation at high temperatures we have only to deal with the obstruction which surfaces offer to combustion, leaving out their dissociating influence at high temperatures which affect most of Deville's results. For that reason Bunsen arrives at much higher dissociation temperatures than Deville, and his mode of experimenting possesses the advantage that it may lead to a proper settlement of the question of temperatures at which dissociation would set in when taking place in a space unencumbered by surfaces.

I should wish some one more experienced than I am with purely physical investigation to make the following experiment:—

Take a narrow tube of about the same size as Bunsen used for his experiments, and a hollow sphere of the same capacity, in both of which Bunsen's experiment should be repeated. The sphere offering less surface than the tube in proportion to the quantity of gas it contains, the dissociation temperature should be found higher in the former than in the latter, if my views are correct. The results that would, in my opinion, be obtained are shown approximately by the red and white coloured surfaces in the diagram (Figs. 7 to 10).

From the temperatures thus obtained, in each case, the real dissociation temperature, if no surfaces were present to influence the result, might be approximately calculated.

Bunsen's method of experimenting, according to my view of the matter, should form the foundation of further research to determine the dissociation temperatures of products of combustion. Even if means were found for eliminating the influence of surfaces, no known material at our disposal could withstand the very high temperature to which the vessels or tubes would be subjected if experiments were carried out according to Deville's method.

That the surfaces of highly heated vessels or tubes either produce, or tend to produce, dissociation, has been corroborated lately by two Russian experimentalists, Menschutkin and Kronowalow. These gentlemen found that dissociation of carbonic acid and other gases was much facilitated when the vessels used for the experiments were filled with material offering rough surfaces, such as asbestos or broken glass.

My view of the theory of dissociation caused or influenced by surfaces, may be given as follows:—Increase of temperature producing expansion of gases will reduce the attractive tendency of the atoms towards one another, or, in other words, diminish their chemical affinity. In the same ratio as the temperature is increased the repelling tendency of the atoms must increase also, until at last decomposition, or what is called dissociation, takes place. This being admitted, it will follow that the adhesive or condensing influence of surfaces on the atoms of the gas, which action will increase at high temperatures, will assist this decomposition by increasing the repelling tendency of the atoms.

Victor Meyer, who at first disputed the accuracy of the results obtained by the two physicists I have mentioned, ultimately accepted them. This circumstance I was very pleased to learn as their experiments confirmed the results I arrived at in practical work with furnaces. Thus the question may be considered nearly settled, the more so as Meyer is himself a great authority in questions of dissociation, having carried out many interesting experiments. Meyer, for instance, proved dissociation by dropping melted platinum into water, and finds that oxygen and hydrogen are evolved from the steam produced. There can be no doubt on this point, but the question arises whether heat is the sole agent that brings about the dissociation of steam in this case. In the first place the dissociating influence of the highly heated surfaces of platinum on steam has to be taken into consideration, and secondly the chemical affinity which platinum has for oxygen, and still more for hydrogen. The same remarks apply to Meyer's experiment of passing steam or carbonic acid through heated platinum tubes, in which case he obtains only traces of dissociation, the temperature being much lower. Other experiments might be mentioned, but none lead to a different conception of the question.

There is one other circumstance connected with dissociation, proved by experiment, which however, requires explanation. It is considered as a sure sign that dissociation is going on when a flame whose temperature is raised becomes longer; this it is said can only be accounted for by dissociation taking place. I agree with this conclusion, but the experiments by which it has been proved have been made, like others referred to, in narrow tubes or passages in which the dissociating action of the heated surfaces must come into play. It is not alone the heat to which the gases are raised that in these cases causes dissociation and increases the length of the flame, but also the influence of the heated surfaces in contact with the combustible gases, more especially if these gases contain hydrocarbons. The extension of the flame is also partly due to the obstruction which the surfaces offer to the recombustion of the dissociated gases through want of space. If the same flame be allowed free development in a space unencumbered by surfaces, as in my radiation furnace, no such extension of its length would be observed; but, on the contrary, it would get shorter with increase of temperature. This action can be best observed in a regenerative gas-burner whose flame is shorter the greater the intensity of the temperature, and therefore of the light produced. On the other hand, flame may be extended almost to any length if conducted through narrow passages; this may be seen in regenerative furnaces which will send the flame to the top of the chimney if the reversing valves are so arranged that the flame, instead of passing through the furnace chamber, is made to burn directly down into the regenerators. No proper combustion can then take place in the brick checkerwork of the regenerative chambers, and the flame will consequently continue to extend until cooled down below a red heat, being ultimately converted into dark smoke; thus in this case, the extensive surfaces offered by regenerators will act both ways, by preventing combustion, and by assisting dissociation.

It will now be understood that regenerative furnaces themselves offer special opportunities for making experiments, most questions being best settled by the results obtained in actual work. If dissociation sets in we see the consequences in want of heat, reduced output, and in destruction of furnace and material. If the causes of dissociation are removed we immediately become aware of the circumstance by a rise in temperature, increased output, longer furnace life, and saving of material.

Similar results may be obtained' with other furnaces, but the beneficial action will not be so great as in the case of the regenerative furnace, because the intensity of heat obtainable in them is much lower.

In applying the principle of heating by radiation, or free development of flame, to boilers, it is necessary to prevent the flame in its active stage of combustion from touching either the sides of the boiler or its brickwork setting. The flame is allowed free space to burn in, and thus good combustion is obtained, after which the products of combustion are brought into intimate contact with the surfaces to be heated. While combustion is going on in the open space heat is transmitted by radiation only, but after active combustion is completed it is transmitted by contact, and it is in this manner that flame must be applied to boilers, and may be applied equally well to nearly all other heating operations.

In heating a boiler the intensity of heat produced is not very great, because the relatively cold surfaces abstract heat from the flame very eagerly, thus preventing its temperature from rising above a certain point which is below that of dissociation. But although no dissociation of the products of combustion can take place in boiler firing, the detrimental effect on combustion of the surfaces of the boiler is nevertheless very great, perhaps, indeed, greater than in any other application of firing and heating. The cold surface of the boiler has the power of extinguishing flame altogether, especially if brought into actual contact with it, because, besides the peculiar influence of surfaces on combustion, the cooling in this case is so great that the necessary temperature for combustion cannot be maintained. Thus it seems clear that heating by radiation must be most advantageous for firing boilers, but particular care should be taken that the products of combustion, as distinguished from the flame, are brought as much as possible into contact with their surfaces. Galloway tubes are preferable to bafflers for this purpose, but it will be necessary to be careful that combustion is complete before the products of combustion are allowed to come into contact with these tubes or bafflers, as otherwise they would interfere with combustion at that point.

In the paper I read before the Iron and Steel Institute, to which I have already referred, I described a boiler heated on the radiation principle, fired with the producer gas used in our regenerative gas furnaces, and with that boiler no smoke is produced. I will now describe a boiler worked on the same principle, fired with common coal, by the use of which great saving of fuel is effected, and very little or no smoke is produced. In this boiler one end of the internal flue is lined with brickwork, and contains an ordinary fire-grate, while the longer part is furnished with rings of cast iron or fire-clay, which prevent the flame from striking on the inner boiler surface. The products of combustion, after leaving the inner flue, where the flame has not come into contact with the boiler plates, are conducted underneath and at the sides of the boiler, and in these channels they may be directed by means of bafflers against the boiler sides and bottom. If the internal flue of the boiler is so long that the flame ceases before reaching its extremity, bafflers, in the form of cones, may also be placed at its far end for the purpose of causing the products of combustion to strike against its sides. Instead of cones, cross tubes of the Galloway type may be used with advantage, but it is absolutely necessary that active combustion should have ceased before the products of combustion come into contact with either bafflers or tubes. In a boiler so arranged and constructed that the flame heats mostly by radiation in its first, and by contact in its second stage, a great saving of fuel is effected, and almost no smoke produced. To avoid altogether the production of smoke in this boiler, the fuel should be charged on the grate in a uniform manner. It is quite impossible to avoid producing smoke and waste if fuel is charged unequally on the grate and at irregular intervals, however well the boiler may have been arranged and constructed. Various kinds of automatic and mechanical coal-feeding arrangements have been suggested, and some have been applied, but none have given full satisfaction. At the London Smoke Abatement Exhibition numerous apparatus of this kind were to be seen, and apparently worked successfully, but when tested at other places, and under different conditions, they have been found wanting, owing to the faulty manner in which the flame and products of combustion were dealt with. These clever appliances would, in my opinion, have worked more satisfactorily if firing and heating had been carried out in them in two successive stages.

There is a very simple way of firing, which I have employed, that may possibly not be quite new, but answers very well, and does not require complicated constructions and appliances, always more or less objectionable. It depends upon the following considerations. When fresh coal is charged upon incandescent fuel, as is the case in the usual mode of firing boilers, the volatile gases of the fresh fuel are rapidly evolved, filling the fire-box to such an extent as to prevent the ingress of air through the grate, and this occurs at the very time the air-supply should be considerably increased. The result is imperfect combustion and consequent waste of the very best combustible gases, viz. the hydro-carbons, which cannot burn for want of air to combine with; free carbon is thus liberated from these gases, and smoke is produced. In order to avoid smoke, and consequent loss of fuel, any sudden production of volatile gases, either during or after firing, must be prevented; and sufficient air should always be introduced, and so distributed, as to burn those gases as quickly as they are produced. This can be done in the following manner:—

Before putting on fresh coal the burning fuel should be pushed back from the front part of the grate and distributed on the incandescent fuel behind, care being taken that this portion of the grate is entirely free from hot fuel. When the front part of the grate has become comparatively cool owing to cold air passing through it, fresh coal is distributed thereon. The freshly charged fuel lying on the cool grate with cold air passing through it will be heated by radiation only, partly from the incandescent fuel behind, partly by the flame from its own gases, and partly by the surrounding hot brickwork. The volatile gases will consequently be liberated at a comparatively slow rate, and will combine with the air which entering through the interstices in the fuel on the cool part of the grate will be evenly distributed over its surface. Gas and air will thus be supplied in nearly the proper proportions for complete combustion of the fuel, and as the production of volatile gases diminishes, the air passing through the front part of the grate, will enter into combustion with the fuel thereon which has been deprived of nearly all its volatile constituents. By means of this simple arrangement the sudden production of a large volume of volatile gases is avoided, and air in a well divided state is always present to consume the gases liberated; thus smokeless combustion and saving of fuel are realised. Care must be taken that the fresh fuel is charged at regular intervals of time and in equal quantities. It still remains to be considered in what manner the clinkers and ashes may be most easily removed, but by the use of a movable pocket at the far end of the grate to collect them in, and a hooked bar to draw it forward at intervals, good practical results would be obtained.

Having dealt so fully with the subject of heating furnaces by radiation, I wish to be allowed to bring before your notice an apparatus for warming rooms by the same means, which I have found to be both satisfactory and economical, and England, I believe, is the country in which it is likely to be fully appreciated, as heating by radiation is almost exclusively used for domestic purposes here.

It must be borne in mind that the regenerative flame radiates much more heat than an ordinary fire or gas flame, because most of the heat which passes away from ordinary flames is in this case employed to increase the temperature, or to accumulate heat, the intensity of the flame, and consequently its radiating power, are thus much increased, or in other words, the heat ordinarily passing away from the flame with the products of combustion is converted into radiant heat.

The apparatus to which I refer is a stove provided with a *regenerative burner*, supplied with *ordinary illuminating or retort gas*, and is intended to warm apartments mainly by the radiated heat of the intensely hot flame produced; it was fully described by me in a paper read at the meeting of the Gas Institute held in Manchester last year. We find in nature that direct heating is effected by radiation exclusively; every organism, as well as all mankind, owe their existence and development to the radiated heat from the sun, and we should try to imitate nature in our methods of obtaining artificial heat. The wind which produces that change of air we require for our well being is another result of the action of the sun. Both heating and ventilation I have endeavoured to supply by means of this stove. Although there is only a small amount of heat passing away from the flame after having heated the air required for combustion it is entirely utilised for the sake of economy; but in cases where economy is not the primary object, but hygienic considerations are paramount, the regenerative gas flame is placed at the foot of the chimney in front of the grate in place of the ordinary coal fire. The background of the stove is of china or other white material, to act as a reflector, by which means the heat, otherwise lost by being radiated backwards or sideways, is recovered to assist in warming

the room. Owing to the high temperature of the regenerative gas flame and the employment of a reflector, which would be quite impossible with an ordinary fire, the economy of fuel attained by the use of this stove, and in a less degree by the use of the chimney fire, is considerable. The maximum consumption of gas in the stove exhibited is about 12 cubic feet per hour. Considering that the gas need not burn constantly, or may be lowered as required, its daily consumption may be set down at 50 to 150 cubic feet of gas for an ordinary room of about 4000 to 5000 cubic feet capacity.

There is still one very important point which remains to be mentioned in connection with a regenerative gas flame of high intensity, provided with a reflecting background; that is, the better distribution of the radiated heat. I find that a room warmed by means of a stove or open fire, such as described, is of a more uniform temperature than when warmed by an ordinary fire or by a gas and coke fire, such as my brother was engaged in introducing into this country shortly before his death.

This, in my opinion, is mainly due to the fact that a source of radiant heat of low intensity but of large surface, sending out its rays at various angles, heats an object in its vicinity very much more than is the case with a smaller source of radiant heat of greater intensity, whose rays strike the object from one direction only, notwithstanding that both sources radiate the same quantity of heat. This action is illustrated by means of the two diagrams exhibited, Figs. 11 and 12, which represent two rooms, the one, Fig. 12, heated by a small flame of high intensity, and the other, Fig. 11, by a large flame of low intensity, both radiating the same quantity of heat. In each room two objects, globes or spheres, are represented, the one close to, and the other at a distance from the source of heat. The object in the one room near to the source having the large heating surface is almost enveloped in rays, while that in the second receives rays only in one direction, the former therefore being much more heated than the latter. This difference does not occur when the two globes at a distance from the two sources of heat are compared. The law that the rays of heat are diminished in the inverse ratio of the square of the distance is only correct as regards small but intense sources of heat, whilst the decrease of radiant heat takes place in a much higher proportion, in the case of large sources of heat of low intensity. This clearly proves that for the purpose of warming rooms by means of radiation, it is important that the heat should be concentrated in an intensely hot focus, as is the case in nature, our earth being warmed in this way by the radiant action of the sun.

From various considerations I am led to believe that the question of sanitary and economical warming is one which commands a great deal of attention in this country. Not many years ago I had to report to my own Government on the Smoke Abatement Exhibition, held in this city, and I understand that a Smoke Abatement Institution has since been inaugurated. There seems to be a general feeling that something will before long have to be used instead of the present fireplace with its smoky chimney, especially now that people are massed together in enormous cities in which cleanliness and pure air are of the greatest importance. Under these circumstances I would venture to draw the attention of authorities in sanitary science to the method of warming dwellings to which I have shortly referred, as resting on a scientific basis, being cleanly and easy of application, demanding little or no attention, and fulfilling all sanitary and economical requirements, and finally as being entirely free from all dissociating influences owing to the free development of the flame.

[F. S.]

WEEKLY EVENING MEETING,

Friday, January 29, 1886.

William Huggins, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

Sir William Thomson, D.C.L. LL.D. F.R.S. *M.R.I.*

Capillary Attraction.

The heaviness of matter had been known for as many thousand years as men and philosophers had lived on the earth, but none had suspected or imagined, before Newton's discovery of universal gravitation, that heaviness is due to action at a distance between two portions of matter. Electrical attractions and repulsions, and magnetic attractions and repulsions, had been familiar to naturalists and philosophers for two or three thousand years. Gilbert, by showing that the earth, acting as a great magnet, is the efficient cause of the compass needle's pointing to the north, had enlarged people's ideas regarding the distances at which magnets can exert sensible action. But neither he nor any one else had suggested that heaviness is the resultant of mutual attractions between all parts of the heavy body and all parts of the earth, and it had not entered the imagination of man to conceive that different portions of matter at the earth's surface, or even the more dignified masses called the heavenly bodies, mutually attract one another. Newton did not himself give any observational or experimental proof of the mutual attraction between any two bodies, of which both are smaller than the moon. The smallest case of gravitational action which was included in the observational foundation of his theory, was that of the moon on the waters of the ocean, by which the tides are produced; but his inductive conclusion that the heaviness of a piece of matter at the earth's surface, is the resultant of attractions from all parts of the earth acting in inverse proportion to squares of distances, made it highly probable that pieces of matter within a few feet or a few inches apart attract one another according to the same law of distance, and Cavendish's splendid experiment verified this conclusion. But now for our question of this evening. Does this attraction between any particle of matter in one body and any particle of matter in another continue to vary inversely as the square of the distance, when the distance between the nearest points of the two bodies is diminished to an inch (Cavendish's experiment does not demonstrate this, but makes it very probable), or to a centimetre, or to the hundred-thousandth of a centimetre, or to the hundred-millionth of a centimetre? Now I dip my finger into this basin of water; you see proved a force of attraction between the finger and the drop hanging from it, and between the matter on the two sides of any horizontal plane you like to imagine through the hanging water. These forces are millions of times greater than what you would calculate from the Newtonian law, on the supposition that water is perfectly homogeneous. Hence either these forces of attraction must, at very small distances, increase enormously more rapidly than according to the Newtonian law, or the substance of water is not homogeneous. We now all know that it is not homogeneous. The Newtonian theory of gravitation is not surer to us now than is the atomic or molecular theory in chemistry and physics; so far, at all events, as its assertion of heterogeneousness in the minute structure of matter apparently homogeneous to our senses and to our most delicate direct instrumental tests. Hence, unless we find heterogeneousness and the Newtonian law of attraction incapable of explaining cohesion and capillary attraction, we are not forced to seek the explanation in a deviation from Newton's law of gravitational force. In a little communication to the Royal Society of Edinburgh twenty-four years ago,* I showed that heterogeneousness does suffice to account for any force of cohesion, however great, provided only we give sufficiently great density to the molecules in the heterogeneous structure.

Nothing satisfactory, however, or very interesting mechanically, seems attainable by any attempt to work out this theory without taking into account the molecular motions which we know to be inherent in matter, and to constitute its heat. But so far as the main phenomena of capillary attraction are concerned, it is satisfactory to know that the complete molecular theory could not but lead to the same resultant action in the aggregate as if water and the solids touching it were each utterly homogeneous to infinite minuteness, and were acted on by mutual forces of attraction sufficiently strong between portions of matter which are exceedingly near one another, but utterly insensible between portions of matter at sensible distances. This idea of attraction insensible at sensible distances (whatever molecular view we may learn, or people not now born may learn after us, to account for the innate nature of the action), is indeed the key to the theory of capillary attraction, and it is to Hawksbee † that we owe it. Laplace took it up and thoroughly worked it out mathematically in a very admirable manner. One part of the theory which he left defective—the action of a solid upon a liquid, and the mutual action between two liquids—was made dynamically perfect by Gauss, and the finishing touch to the mathematical theory was given by Neumann in stating for liquids the rule corresponding to Gauss's rule for angles of contact between liquids and solids.

Gauss, expressing enthusiastic appreciation of Laplace's work, adopts the same fundamental assumption of attraction sensible only at insensible distances, and, while proposing as chief object to complete the part of the theory not worked out by his predecessor, treats the dynamical problem afresh in a remarkably improved manner, by founding it wholly upon the principle of what we now call potential energy. Thus, though the formulas in which he expresses mathematically his ideas are scarcely less alarming in appearance than those of Laplace, it is very easy to translate them into words by which the whole theory will be made perfectly intelligible to persons who imagine themselves incapable of understanding sextuple integrals. Let us place ourselves conveniently at the centre of the earth so as not to be disturbed by gravity. Take now two portions of water, and let them be shaped over a certain area of each, call it A for the one, and B for the other, so that when put together they will fit perfectly throughout these areas. To save all trouble in manipulating the supposed pieces of water, let them become for a time perfectly rigid, without, however, any change in their mutual attraction. Bring them now together till the two surfaces A and B come to be within the one-hundred-thousandth of an inch apart, that is, the forty-thousandth of a centimetre, or 250 micro-millimetres (about half the wave-length of green light). At so great a distance the attraction is quite insensible: we may feel very confident that it differs, by but a small percentage, from the exceedingly small force of attraction which we should calculate for it according to the Newtonian law, on the supposition of perfect uniformity of density in each of the attracting bodies. Well-known phenomena of bubbles, and of watery films wetting solids, make it quite certain that the molecular attraction does not become sensible until the distance is much less than 250 micro-millimetres. From the consideration of such phenomena Quincke (Pogg. Ann., 1869) came to the conclusion that the molecular attraction does become sensible at distances of about 50 micro-millimetres. His conclusion is strikingly confirmed by the very important discovery of Reinold and Rücker* that the black film, always formed before an undisturbed soap bubble breaks, has a uniform or nearly uniform thickness of about 11 or 12 micro-millimetres. The abrupt commencement, and the permanent stability, of the black film demonstrate a proposition of fundamental importance in the molecular theory:—The tension of the film, which is sensibly constant when the thickness exceeds 50 micro-millimetres, diminishes to a minimum, and begins to increase again when the thickness is diminished to 10 micro-millimetres. It seems not possible to explain this fact by any imaginable law of force between the different portions of the film supposed homogeneous, and we are forced to the conclusion that it depends upon molecular heterogeneousness. When the homogeneous molar theory is thus disproved by observation, and its assumption of a law of attraction augmenting more rapidly than according to the Newtonian law when the distance becomes less than 50 micro-millimetres is proved to be insufficient, may we not go farther and say that it is unnecessary to assume any deviation from the Newtonian law of force varying inversely as the square of the distance continuously from the millionth of a micro-millimetre to the distance of the remotest star or remotest piece of matter in the universe; and, until we see how gravity itself is to be explained, as Newton and Faraday thought it must be explained, by some continuous action of intervening or surrounding matter, may we not be temporarily satisfied to explain capillary attraction merely as Newtonian attraction intensified in virtue of intensely dense molecules movable among one another, of which the aggregate constitutes a mass of liquid or solid.

---

* Proceedings of the Royal Society of Edinburgh, April 21, 1862 (vol. iv.).

† Royal Society Transactions, 1709–13.

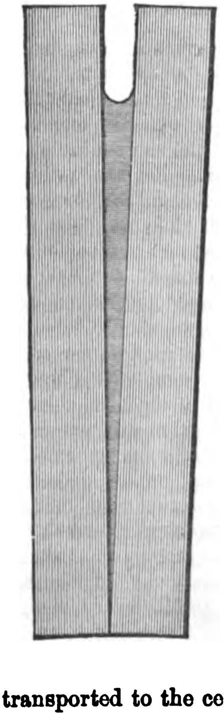
---

* Proc. Roy. Soc., June 21, 1877; Trans. Roy. Soc., April 19, 1883.

But now for the present, and for the rest of this evening, let us dismiss all idea of molecular theory, and think of the molar theory pure and simple, of Laplace and Gauss. Returning to our two pieces of rigidified water left at a distance of 250 micro-millimetres from one another. Holding them in my two hands, I let them come nearer and nearer until they touch all along the surfaces A and B. They begin to attract one another with a force which may be scarcely sensible to my hands when their distance apart is 50 micro-millimetres, or even as little as 10 micro-millimetres; but which certainly becomes sensible when the distance becomes one micro-millimetre, or the fraction of a micro-millimetre; and enormous, hundreds or thousands of kilogrammes' weight, before they come into absolute contact. I am supposing the area of each of the opposed surfaces to be a few square centimetres. To fix the ideas, I shall suppose it to be exactly thirty square centimetres. If my sense of force were sufficiently metrical I should find that the work done by the attraction of the rigidified pieces of water in pulling my two hands together was just about 4½ centimetre-grammes. The force to do this work, if it had been uniform throughout the space of 50 micro-millimetres (five-millionths of a centimetre) must have been 900,000 grammes weight, that is to say, nine-tenths of a ton. But in reality it is done by a force increasing from something very small at the distance of 50 micro-millimetres to some unknown greatest amount. It may reach a maximum before absolute contact, and then begin to diminish, or it may increase and increase up to contact, we cannot tell which. Whatever may be the law of variation of the force, it is certain that throughout a small part of the distance it is considerably more than one ton. It is possible that it is enormously more than one ton, to make up the ascertained amount of work of 4½ centimetre-grammes performed in a space of 50 micro-millimetres.

But now let us vary the circumstances a little. I take the two pieces of rigidified water, and bring them to touch at a pair of corresponding points in the borders of the two surfaces A and B, keeping the rest of the surfaces wide asunder (see Fig. 1). The work done on my hands in this proceeding is infinitesimal. Now, without at all altering the law of attractive force, let a minute film of the rigidified water become fluid all over each of the surfaces A and B; you see exactly what takes place. The pieces of matter I hold in my hands are not the supposed pieces of rigidified water. They are glass, with the surfaces A and B thoroughly cleaned and wetted all over each with a thin film of water. What you now see taking place is the same as what would take place if things were exactly according to our ideal supposition. Imagine, therefore, that these are really two pieces of water, all rigid, except the thin film on each of the surfaces A and B, which are to be put together. Remember also that the Royal Institution, in which we are met, has been, for the occasion, transported to the centre of the earth so that we are not troubled in any way by gravity. You see we are not troubled by any trickling down of these liquid films—but I must not say *down*, we have no up and down here. You see the liquid film does not trickle along these surfaces towards the table, at least you must imagine that it does not do so. I now turn one or both of these pieces of matter till they are so nearly in contact all over the

FIG. 1.



surfaces A and B, that the whole interstice becomes filled with water. My metrical sense of touch tells me that exactly 4½ centimetre-grammes of work has again been done; this time, however, not by a very great force, through a space of less than 50 micro-millimetres, but by a very gentle force acting throughout the large space of the turning or folding-together motion which you have seen, and now see again. We know, in fact, by the elementary principle of work done in a conservative system, that the work done in the first case of letting the two bodies come together directly, and in the second case of letting them come together by first bringing two points into contact and then folding them together, must be the same, and my metrical sense of touch has merely told me in this particular sense what we all know theoretically must be true in every case of proceeding by different ways to the same end from the same beginning.

Now in this second way we have, in performing the folding motion, allowed the water surface to become less by 60 square centimetres. It is easily seen that, provided the radius of curvature in every part of the surface exceeds one or two hundred times the extent of distance to which the molecular attraction is sensible, or, as we may say practically, provided the radius of curvature is everywhere greater than 5000 micro-millimetres (that is, the two-hundredth of a millimetre), we should have obtained this amount of work with the same diminution of water-surface, however performed. Hence our result is that we have found 4·5/60 (or 3/40) of a centimetre-gramme of work per square centimetre of diminution of surface. This is precisely the result we should have had if the water had been absolutely deprived of the attractive force between water and water, and its whole surface had been coated over with an infinitely thin contractile film possessing a uniform contractile force of 3/40 of a gramme weight, or 75 milligrammes, per lineal centimetre.

It is now convenient to keep to our ideal film, and give up thinking of what, according to our present capacity for imagining molecular action, is the more real thing—namely, the mutual attraction between the different portions of the liquid. But do not, I entreat you, fall into the paradoxical habit of thinking of the surface film as other than an ideal way of stating the resultant effect of mutual attraction between the different portions of the fluid. Look, now, at one of the pieces of water ideally rigidified, or, if you please, at the two pieces put together to make one. Remember we are at the centre of the earth. What will take place if this piece of matter resting in the air before you suddenly ceases to be rigid? Imagine it, as I have said, to be enclosed in a film everywhere tending to contract with a force equal to 3/40 of a gramme or 75 milligrammes weight per lineal centimetre. This contractile film will clearly press most where the convexity is greatest. A very elementary piece of mathematics tells us that on the rigid convex surface which you see, the amount of its pressure per square centimetre will be found by multiplying the sum * of the curvatures in two mutually-perpendicular normal sections, by the amount of the force per lineal centimetre. In any place where the surface is concave the effect of the surface tension is to suck outwards—that is to say, in mathematical language, to exert negative pressure inwards. Now, suppose in an instant the rigidity to be annulled, and the piece of glass which you see, still undisturbed by gravity, to become water. The instantaneous effect of these unequal pressures over its surface will be to set it in motion. If it were a perfect fluid it would go on vibrating for ever with wildly-irregular vibrations, starting from so rude an initial shape as this which I hold in my hand. Water, as any other liquid, is in reality viscous, and therefore the vibrations will gradually subside, and the piece of matter will come to rest in a spherical figure, slightly warmed as the result of the work done by the forces of mutual attraction by which it was set in motion from the initial shape. The work done by these forces during the change of the body from any one shape to any other is in simple proportion to the diminution of the whole surface area; and the configuration of equilibrium, when there is no disturbance from gravity, or from any other solid or liquid body, is the figure in which the surface area is the smallest possible that can enclose the given bulk of matter.

I have calculated the period of vibration of a sphere of water † (a dew-drop!) and find it to be $\frac{1}{4} a^{\frac{3}{2}}$, where a is the radius measured in centimetres; thus—

For a radius of	$\frac{1}{4}$	cm. the	period is	$\frac{1}{32}$	second.	
,,	1	,,	,,	$\frac{1}{4}$	,,	
,,	2·54	,,	,,	1	,,	
,,	4	,,	,,	2	,,	
,,	16	,,	,,	16	,,	
,,	36	,,	,,	~~36~~	,,	; ✓
,,	1407	,,	,,	13,200	,,	

The dynamics of the subject, so far as a single liquid is concerned, is absolutely comprised in the mathematics without symbols which I have put before you. Twenty pages covered with sextuple integrals could tell us no more.

Hitherto we have only considered mutual attraction between the parts of two portions of one and the same liquid—water for instance. Consider, now, two different kinds of liquid: for instance, water and carbon disulphide (which, for brevity, I shall call sulphide). Deal with them exactly as we dealt with the two pieces of water. I need

---

* This sum for brevity I henceforth call simply "the curvature of the surface" at any point.

† See paper by Lord Rayleigh in Proceedings of the Royal Society, No. 196, May 5, 1879.

not go through the whole process again; the result is obvious. Thirty times the excess of the sum of the surface-tensions of the two liquids separately, above the tension of the interface between them, is equal to the work done in letting the two bodies come together directly over the supposed area of thirty square centimetres. *Hence the interfacial tension per unit area of the interface is equal to the excess of the sum of the surface-tensions of the two liquids separately, above the work done in letting the two bodies come together directly so as to meet in a unit area of each.* In the particular case of two similar bodies coming together into perfect contact, the interfacial tension must be zero, and therefore the work done in letting them come together over a unit area must be exactly equal to twice the surface-tension; which is the case we first considered.

If the work done between two different liquids in letting them come together over a small area, exceeds the sum of the surface-tensions, the interfacial tension is negative. The result is an instantaneous puckering of the interface, as the commencement of diffusion and the well-known process of continued inter-diffusion follows.

Consider next the mutual attraction between a solid and a liquid. Choose any particular area of the solid, and let a portion of the surface of the liquid be preliminarily shaped to fit it. Let now the liquid, kept for the moment rigid, be allowed to come into contact over this area with the solid. The amount by which the work done per unit area of contact falls short of the surface-tension of the liquid is equal to the interfacial tension of the liquid. If the work done per unit area is exactly equal to the free-surface tension of the liquid, the interfacial tension is zero. In this case the surface of the liquid when in equilibrium at the place of meeting of liquid and solid is at right angles to the surface of the solid. The angle between the free surfaces of liquid and solid is acute or obtuse according as the interfacial tension is positive or negative; its cosine being equal to the interfacial tension divided by the free-surface tension. The greatest possible value the interfacial tension can have is clearly the free-surface tension, and it reaches this limiting value only in the, not purely static, case of a liquid resting on a solid of high thermal conductivity, kept at a temperature greatly above the boiling-point of the liquid; as in the well-known phenomena to which attention has been called by Leidenfrost and Boutigny. There is no such limit to the absolute value of the interfacial tension when negative, but its absolute value must be less than that of the free-surface tension to admit of equilibrium at a line of separation between liquid and solid. If minus the interfacial tension is exactly equal to the free-surface tension, the angle between the free surfaces at the line of separation is exactly 180°. If minus the interfacial tension exceeds the free-surface tension, the liquid runs all over the solid, as, for instance, water over a glass plate which has been very perfectly cleansed. If for a moment we leave the centre of the earth, and suppose ourselves anywhere else in or on the earth, we find the liquid running up, against gravity, in a thin film over the upper part of the containing vessel, and leaving the interface at an angle of 180° between the free surface of the liquid, and the surface of the film adhering to the solid above the bounding line of the free liquid surface. This is the case of water contained in a glass vessel, or in contact with a piece of glass of any shape, provided the surface of the glass be very perfectly cleansed.

When two liquids which do not mingle, that is to say, two liquids of which the interfacial tension is positive, are placed in contact and left to themselves undisturbed by gravity (in our favourite Laboratory in the centre of the earth suppose), after performing vibrations subsiding in virtue of viscosity, the compound mass will come to rest, in a configuration consisting of two intersecting segments of spherical surfaces constituting the outer boundary of the two portions of liquid, and a third segment of spherical surface through their intersection constituting the interface between the two liquids. These three spherical surfaces meet at the same angles as three balancing forces in a plane whose magnitudes are respectively the surface tensions of the outer surfaces of the two liquids and the tension of their interface. Figs. 2 to 5 (pp. 492, 493) illustrate these configurations in the case of bisulphide of carbon and water for several different proportions of the volumes of the two liquids. (In the figures the dark shading represents water in each case.) When the volume of each liquid is given, and the angles of meeting of the three surfaces are known, the problem of describing the three spherical surfaces is clearly determinate. It is an interesting enough geometrical problem.

If we now for a moment leave our gravitationless laboratory, and, returning to the Theatre of the Royal Institution, bring our two masses of liquid into contact, as I now do in this glass bottle, we have the one liquid floating upon the other, and the form assumed by the floating liquid may be learned, for several different cases, from the phenomena exhibited in these bottles and glass beakers, and shown on an enlarged scale in these two diagrams (Figs. 6 to 8, p. 494); which represent bisulphide of carbon floating on the surface of sulphate of zinc, and in this case (Fig. 8) the bisulphide of carbon drop is of nearly the maximum size capable of floating. Here is the bottle whose contents are represented in Fig. 8, and we shall find that a very slight vertical disturbance serves to submerge the mass of bisulphide of carbon. There now it has sunk, and we shall find when its vibrations have ceased that the bisulphide of carbon has taken the form of a large sphere supported within the sulphate of zinc. Now, remembering that we are again at the centre of the earth, and that gravity does not hinder us, suppose the glass matter of the bottle suddenly to become liquid sulphate of zinc, this mass would become a compound sphere like the one shown on that diagram (Fig. 3), and

Fig. 2.

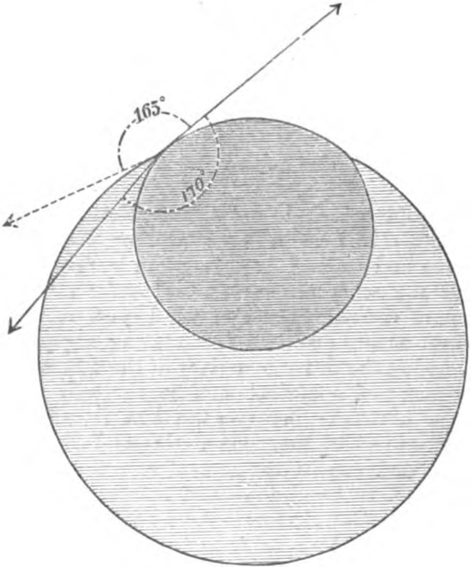


Fig. 3.

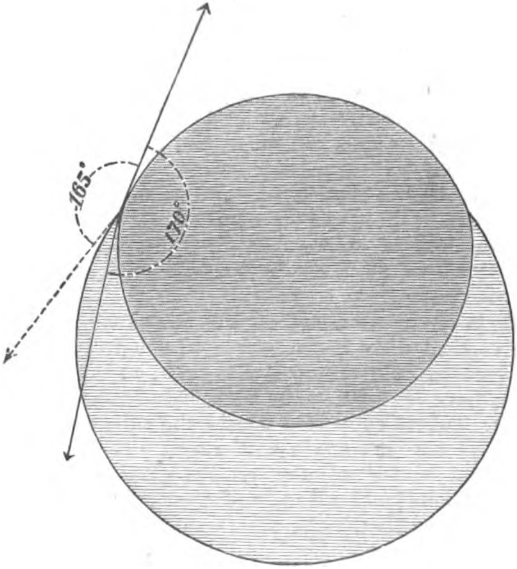


FIG. 4.

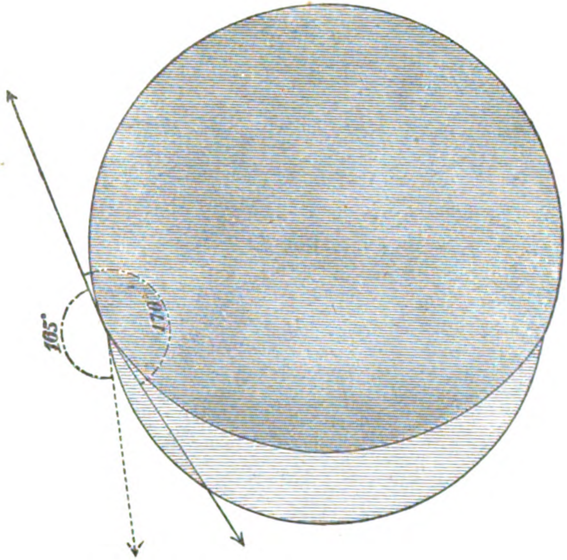
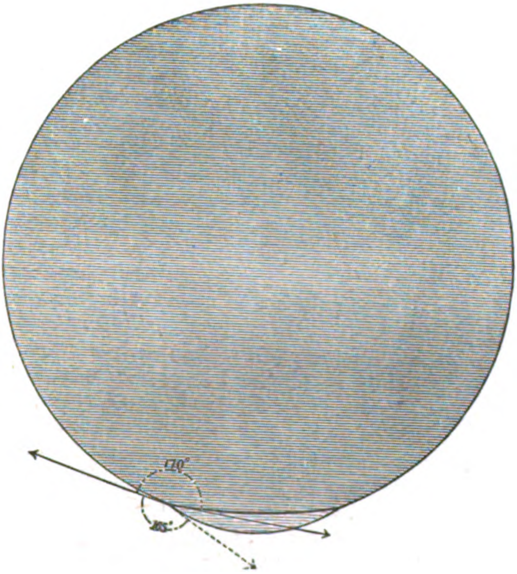


FIG. 5.



would have a radius of about 8 centimetres. If it were sulphate of zinc alone, and of this magnitude, its period of vibration would be about 5½ seconds.

Fig. 6.

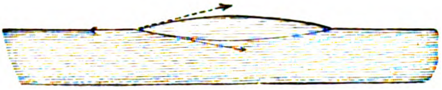


Fig. 7.

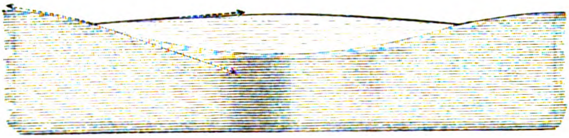


Fig. 8.

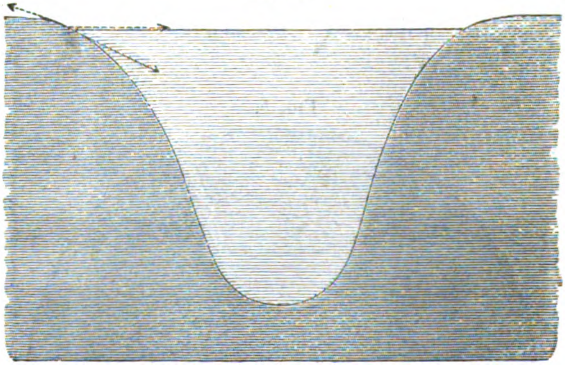
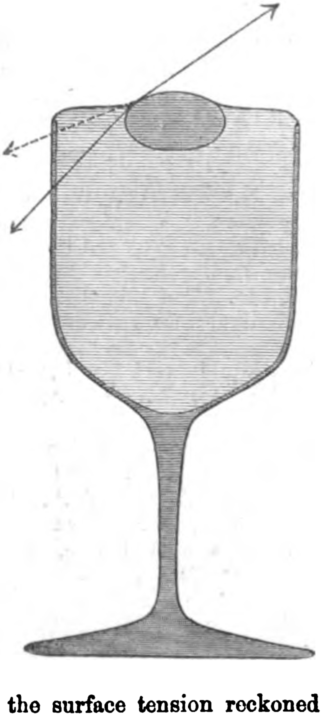


Fig. 9 shows a drop of sulphate of zinc floating on a wine-glassful of bisulphide of carbon.

In observing the phenomena of two liquids in contact, I have found it very convenient to use sulphate of zinc (which I find, by experiment, has the same free-surface tension as water) and bisulphide of carbon; as these liquids do not mix when brought together, and, for a short time at least, there is no chemical interaction between them. Also, sulphate of zinc may be made to have a density less than, or equal to, or greater than, that of the bisulphide, and the bisulphide may be coloured to a more or less deep purple tint by iodine, and this enables us easily to observe drops of any one of these liquids on the other. In the three bottles now before you the clear liquid is sulphate of zinc—in one bottle it has a density less than, in another equal to, and in the third greater than, the density of the sulphide—and you see how, by means of the coloured sulphide, all the phenomena of drops resting upon or floating within a liquid into which they do not diffuse may be observed, and, under suitable arrangements, quantitatively estimated.

When a liquid under the influence of gravity is supported by a solid, it takes a configuration in which the difference of curvature of the free surface at different levels is equal to the difference of levels divided by the surface tension reckoned in terms of weight of unit bulk of the liquid as unity; and the free surface of the liquid leaves the free surface of the solid at the angle whose cosine is, as stated above, equal to the interfacial tension divided by the free-surface tension, or at an angle of 180° in any case in which minus the interfacial tension exceeds the free-surface tension. The surface equation of equilibrium and the boundary conditions thus stated in words, suffice fully to determine the configuration when the volume

Fig. 9.



of the liquid and the shape and dimensions of the solid are given. When I say determine, I do not mean unambiguously. There may of course be a multiplicity of solutions of the problem; as, for instance, when the solid presents several hollows in which, or projections hanging from which, portions of the liquid, or in or hanging from any one of which the whole liquid, may rest.

When the solid is symmetrical round a vertical axis, the figure assumed by the liquid is that of a figure of revolution, and its form is determined by the equation given above in words. A general solution of this problem by the methods of the differential and integral calculus transcends the powers of mathematical analysis, but the following simple graphical method of working out what constitutes mathematically a complete solution, occurred to me a great many years ago.

Draw a line to represent the axis of the surface of revolution. This line is vertical in the realisation now to be given, and it or any line parallel to it will be called vertical in the drawing, and any line perpendicular to it will be called horizontal. The distance between any two horizontal lines in the drawing will be called *difference of levels.*

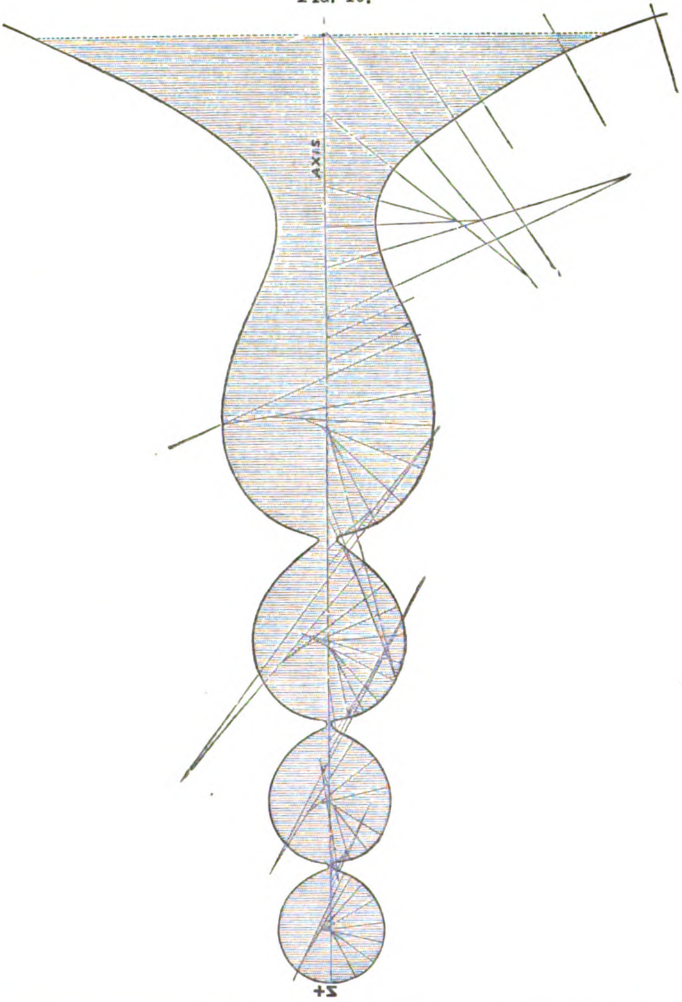
Through any point, N, of the axis draw a line, N P, cutting it at any angle. With any point, O, as centre on the line N P, describe a very small circular arc through P P′, and let N′ be the point in which the line of O P′ cuts the axis. Measure N P, N′ P′, and the difference of levels between P and P′. Denoting this last by δ, and taking a as a linear parameter, calculate the value of

$$\left(\frac{\delta}{a^2}+\frac{1}{\mathrm{OP}}+\frac{1}{\mathrm{NP}}-\frac{1}{\mathrm{N'P'}}\right)^{-1}$$

Take this length on the compasses, and putting the pencil point at P′, place the other point at O′ on the line P′ N′, and with O′ as centre, describe a small arc, P′ P″. Continue the process according to the same rule, and the successive very small arcs so drawn will constitute a curved line, which is the generating line of the surface of revolution inclosing the liquid, according to the conditions of the special case treated.

This method of solving the capillary equation for surfaces of revolution remained unused for fifteen or twenty years, until in 1874 I placed it in the hands of Mr. John Perry (now Professor of Mechanics at the City and Guilds Institute), who was then attending the Natural Philosophy Laboratory of Glasgow University. He worked out the problem with great perseverance and ability, and the result of his labours was a series of skilfully executed drawings representing a large variety of cases of the capillary surfaces of revolution. These drawings, which are most instructive and valuable, I have not yet been able to prepare for publication, but the most characteristic of them have been reproduced on an enlarged scale, and are now on the screen before you.* Three of these diagrams, those to which I am now pointing (Figs. 10, 11, and 12), illustrate strictly theoretical solutions—that is to say, the curves there shown

FIG. 10.



---

* The diagrams here referred to are now published in Figs. 10 to 24 of the present report of the lecture at the Royal Institution. These figures are accurate copies of Mr. Perry's original drawings, and I desire to acknowledge the great care and attention which Mr. Cooper, engraver to *Nature*, has given to the work.

FIG. 11.

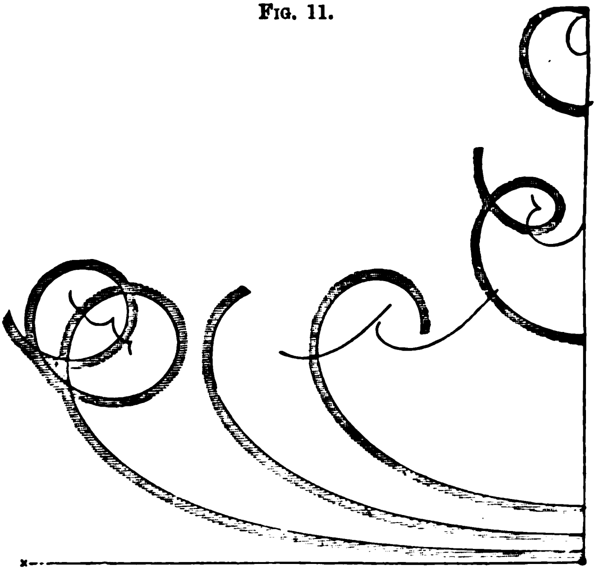
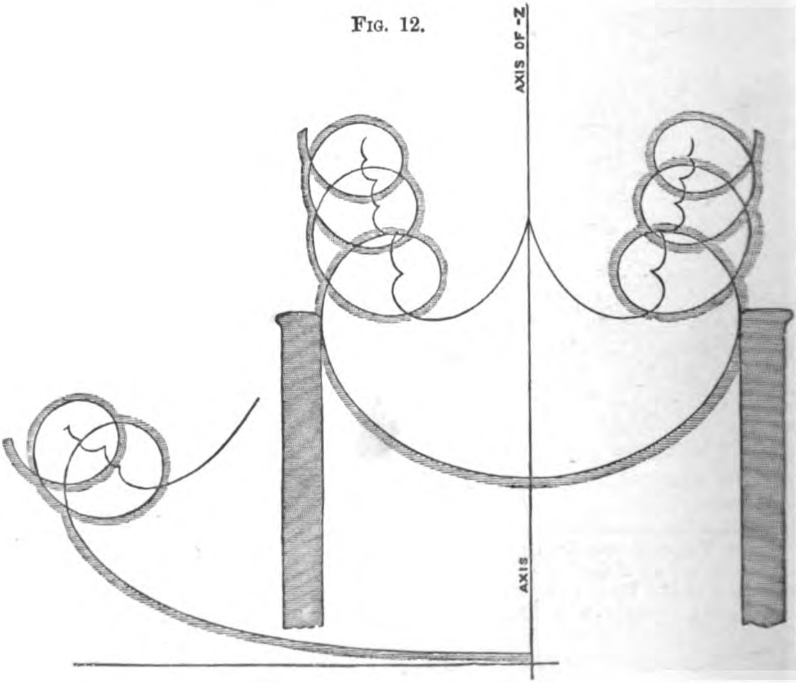


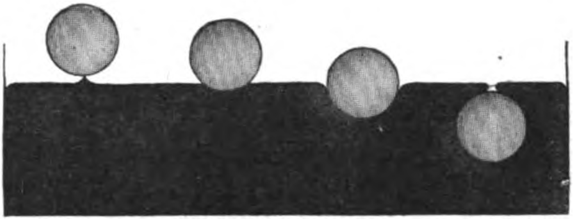
FIG. 12.


AXIS OF -Z
AXIS

do not represent real capillary surfaces—but these mathematical extensions of the problem, while most interesting and instructive, are such as cannot be adequately treated in the time now at my disposal.

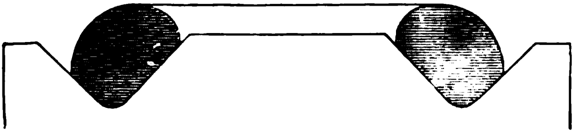
In these other diagrams, however (Figs. 13 to 28), we have certain portions of the curves taken to represent real capillary surfaces shown in section. In Fig. 13 a solid sphere is shown in four different positions in contact with a mercury surface; and again, in Fig. 14 we have a section of the form assumed by mercury resting in a circular V-groove. Figs. 15 to 28 (pp. 500–502) show water-surfaces under different conditions as to capillarity; the scale of the drawings for each set of figures is shown by a line the length of which represents 1 centimetre; the dotted horizontal lines indicate the positions of the free water-level. The drawings are sufficiently explicit to require no further reference here save the remark that *water* is represented by the lighter shading, and *solid* by the darker.

FIG. 13.



Mercury in contact with solid spheres (say of glass).

FIG. 14.



Sectional view of circular V-groove containing mercury.

We have been thinking of our pieces of rigidified water as becoming suddenly liquefied, and conceiving them inclosed within ideal contractile films; I have here an arrangement by which I can exhibit on an enlarged scale a pendant drop, inclosed not in an *ideal* film, but in a *real* film of thin sheet indiarubber. The apparatus which you see here suspended from the roof is a stout metal ring of 60 centimetres diameter, with its aperture closed by a sheet of india-

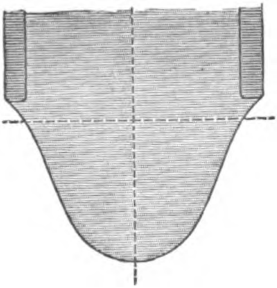


Fig. 15.

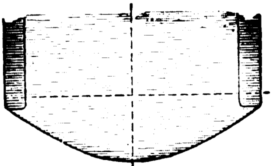


Fig. 16.

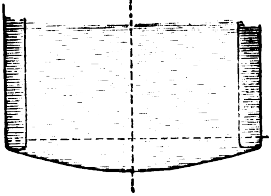


Fig. 17.

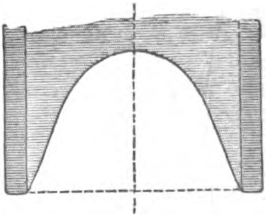


Fig. 18.

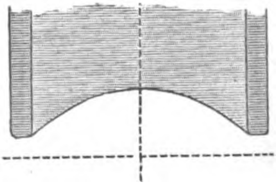


Fig. 19.

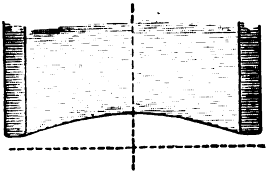


Fig. 20.

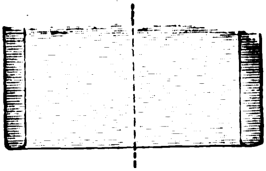


Fig. 21.

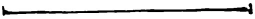
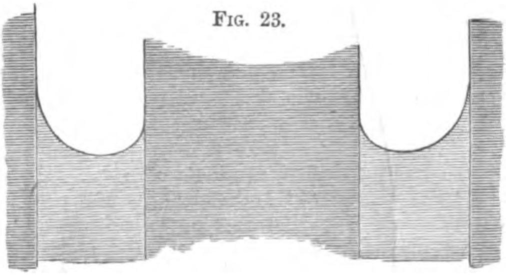


Fig. 22.

Water in glass tubes, the internal diameter of which may be found from Fig. 22, which represents a length of one centimetre.

Fig. 23.



Water resting in the space between a solid cylinder and a concentric hollow cylinder.

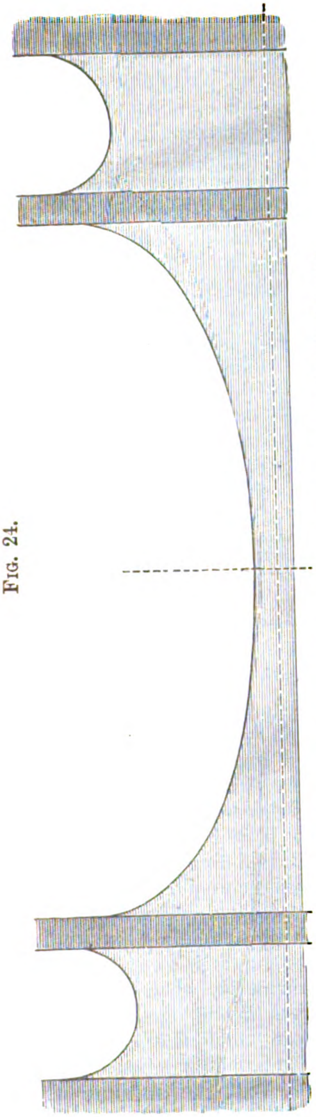


FIG. 24.

Water resting in two co-axial cylinders; scale is represented by Fig. 28.

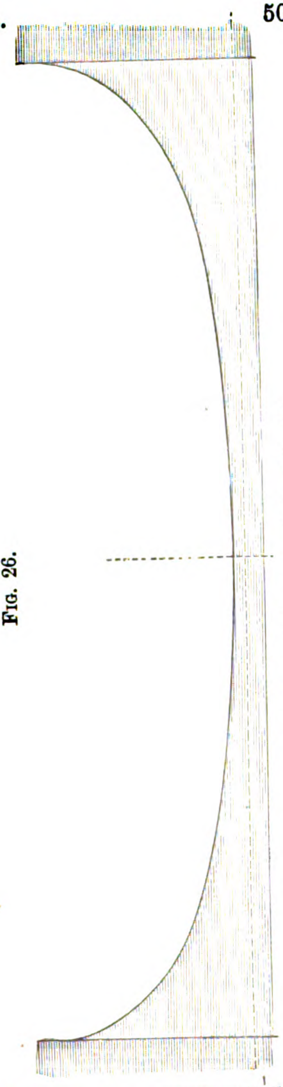


FIG. 26.

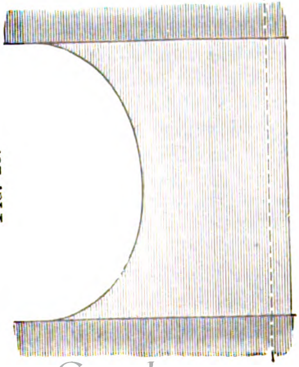
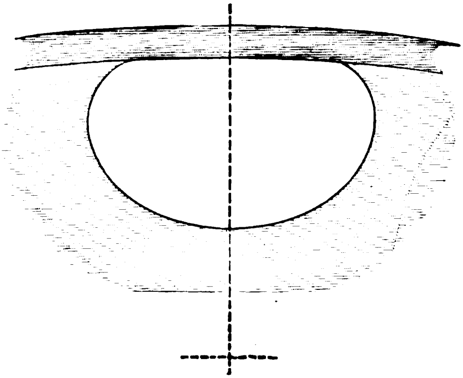


FIG. 25.

Water resting in hollow cylinders (tubes); scale is represented by Fig. 28.

rubber tied to it all round, stretched uniformly in all directions, and as tightly as could be done without special apparatus for stretching it and binding it to the ring when stretched.

FIG. 27.



Section of the air-bubble in a level tube filled with water, and bent so that its axis is part of a circle of large radius; scale is represented in Fig. 28.

FIG. 28.

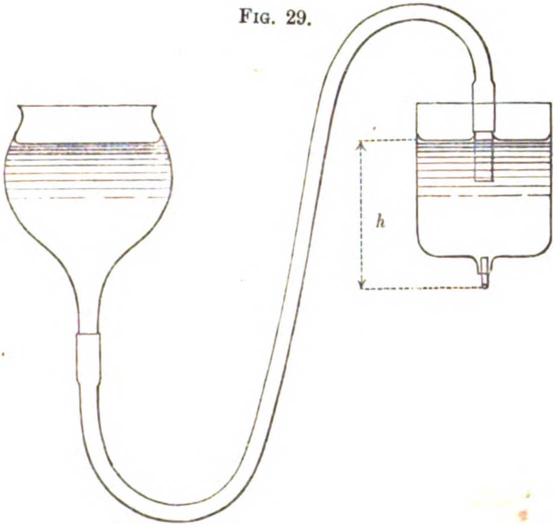
Represents a length of one centimetre for Figs. 24–27.

I now pour in water, and we find the flexible bottom assuming very much the same shape as the drop which you saw hanging from my finger after it had been dipped into and removed from the vessel of water (see Fig. 16). I continue to pour in more water, and the form changes gradually and slowly, preserving meanwhile the general form of a drop such as is shown in Fig. 16, until, when a certain quantity of water has been poured in, a sudden change takes place. The sudden change corresponds to the breaking away of a real drop of water from, for example, the mouth of a tea-urn, when the stopcock is so nearly closed that a very slow dropping takes place. The drop in the indiarubber bag, however, does not fall away, because the tension of the indiarubber increases enormously when the indiarubber is stretched. The tension of the real film at the surface of a drop of water remains constant, however much the surface is stretched, and therefore the drop breaks away instantly when enough of water has been supplied from above to feed the drop to the greatest volume that can hang from the particular size of tube which is used.

I now put this siphon into action, gradually drawing off some of the water, and we find the drop gradually diminishes until a sudden change again occurs and it assumes the form we observed (Fig. 16) when I first poured in the water. I instantly stop the action of the siphon, and we now find that the great drop has two possible forms of stable equilibrium, with an unstable form intermediate between them. Here is an experimental proof of this statement. With the drop in its higher stable form I cause it to vibrate so as alternately to decrease and increase the axial length, and you see that when the vibrations are such as to cause the increase of length to reach a certain limit there is a sudden change to the lower stable form, and we may now leave the mass performing small vibrations about that lower form. I now increase these small vibrations, and we see that, whenever, in one of the upward (increasing) vibrations, the contraction of axial length reaches the limit already referred to, there is again a sudden change, which I promote by gently lifting with my hands, and the mass assumes the higher stable form, and we have it again performing small vibrations about this form.

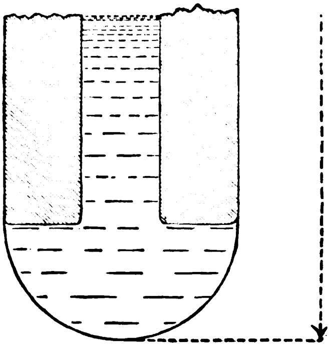
The two positions of stable equilibrium, and the one of unstable intermediate between them, is a curious peculiarity of the hydrostatic problem presented by the water supported by indiarubber in the manner of the experiment.

FIG. 29.



I have here a simple arrangement of apparatus (Figs. 29 and 30) by which, with proper optical aids, such as a cathetometer and a microscope, we can make the necessary measurements on real drops of water or other liquid, for the purpose of determining the values of the capillary constants. For stability the drop hanging from the open tube should be just less than a hemisphere, but for convenience it is shown, as in the enlarged drawing of the nozzle (Fig. 30),

FIG. 30.



exactly hemispherical. By means of the siphon the difference of levels, h, between the free level surface of the water in the vessel to which the nozzle is attached, and the lowest point in the drop hanging from the nozzle, may be varied, and corresponding measurements taken of h and of r, the radius of curvature of the drop at its lowest point. This measurement of the curvature of the drop is easily made with somewhat close accuracy, by known microscopic methods. The surface-tension T of the liquid is calculated from the radius, r, and the observed difference of levels, h, as follows :—

$$\frac{2\text{T}}{r} = h;$$

for example, if the liquid taken be water, with a free-surface tension of 75 milligrammes per centimetre, and $r = \cdot 05$ cm., h is equal to 3 centimetres.

Many experiments may be devised to illustrate the effects of surface-tension when two liquids, of which the surface-tensions are widely different, are brought into contact with each other. Thus we may place on the surface of a thin layer of water, wetting uniformly the surface of a glass plate or tray, a drop of alcohol or ether, and so cause the surface-tension of the liquid layer to become smaller in the region covered by the alcohol or ether. On the other hand, from a surface-layer of alcohol largely diluted with water we may arrange to withdraw part of the alcohol at one particular place by promoting its rapid evaporation, and thereby increase the surface-tension of the liquid layer in that region by diminishing the percentage of alcohol which it contains.

In this shallow tray, the bottom of which is of ground glass resting on white paper, so as to make the phenomena to be exhibited more easily visible, there is a thin layer of water coloured deep blue with aniline; now, when I place on the water-surface a small quantity of alcohol from this fine pipette, observe the effect of bringing the alcohol-surface, with a surface-tension of only 25·5 dynes per lineal centimetre, into contact with the water-surface, which has a tension of 75 dynes per lineal centimetre. See how the water pulls back, as it were, all round the alcohol, forming a circular ridge surrounding a hollow, or small crater, which gradually widens and deepens until the glass plate is actually laid bare in the centre, and the liquid is heaped up in a circular ridge around it. Similarly, when I paint with a brush a streak of alcohol across the tray, we find the water drawing back on each side from the portion of the tray touched with the brush. Now, when I incline the glass tray, it is most interesting to observe how the coloured water with its slight admixture of alcohol flows down the incline—first in isolated drops, afterwards joining together into narrow continuous streams.

These and other well-known phenomena, including that interesting one, "tears of strong wine," were described and explained in a paper "On Certain Curious Motions Observable on the Surfaces of Wine and other Alcoholic Liquors," by my brother, Prof. James Thomson, read before Section A of the British Association at the Glasgow meeting of 1855.

I find that a solution containing about 25 per cent. of alcohol shows the "tears" readily and well, but that they cannot at all be produced if the percentage of alcohol is considerably smaller or considerably greater than 25. In two of those bottles the coloured solution contains respectively 1 per cent. and 90 per cent. of alcohol, and in them you see it is impossible to produce the "tears"; but when I take this third bottle, in which the coloured liquid contains 25 per cent. of alcohol, and operate upon it, you see—there—the "tears" begin to form at once. I first incline and rotate the bottle so as to wet its inner surface with the liquid, and then, leaving it quite still, I remove the stopper, and withdraw by means of this paper tube the mixture of air and alcoholic vapour from the bottle and allow fresh air to take its place. In this way I promote the evaporation of alcohol from all liquid surfaces within the bottle, and where the liquid is in the form of a thin film it very speedily loses a great part of its alcohol. Hence the surface-tension of the thin film of liquid on the interior wall of the bottle comes to have a greater and greater value than the surface-tension of the mass of liquid in the bottom, and where these two liquid surfaces, having different surface-tensions, come together we have the phenomena of "tears." There, as I hasten the evaporation, you see the horizontal ring rising up the side of the bottle, and afterwards collecting into drops which slip down the side and give a fringe-like appearance to the space through which the rising ring has passed.

These phenomena may also be observed by using, instead of alcohol, ether, which has a surface-tension equal to about three-fourths of that of alcohol. In using ether, however, this very curious effect may be seen.* I dip the brush into the ether, and hold it near to but not touching the water-surface. Now I see a hollow formed, which becomes more or less deep according as the brush is nearer to or farther from the normal water surface, and it follows the brush about as I move it so.

Here is an experiment showing the effect of heat on surface-tension. Over a portion of this tin plate there is a thin layer of resin. I lay the tin plate on this hot copper cylinder, and we at once see the fluid resin drawing back from the portion of the tin plate directly over the end of the heated copper cylinder, and leaving a circular space on the surface of the tin plate almost clear of resin, showing how very much the surface-tension of hot resin is less than that of cold resin.

Note of January 30, 1886.—The equations (8) and (9) on p. 59 of Clerk-Maxwell's article on "Capillary Attraction" in the ninth edition of the "Encyclopædia Britannica" do not contain terms depending on the mutual action between the two liquids, and the concluding expression (10), and the last small print paragraph of the page are wholly vitiated by this omission. The paragraph immediately following equation (10) is as follows:—

"If this quantity is positive, the surface of contact will tend to contract, and the liquids will remain distinct. If, however, it were negative, the displacement of the liquids which tends to enlarge the surface of contact would be aided by the molecular forces, so that the liquids, if not kept separate by gravity, would become thoroughly mixed. No instance, however, of a phenomenon of this kind has been discovered, for those liquids which mix of themselves do so by the process of diffusion, which is a molecular motion, and not by the spontaneous puckering and replication of the boundary surface as would be the case if T were negative."

It seems to me that this view is not correct; but that on the contrary there is this "puckering" as the *very beginning* of diffusion. What I have given in the lecture as reported in the text above seems to me the right view of the case as regards diffusion in relation to interfacial tension.

It may also be remarked that Clerk-Maxwell, in the large print paragraph of p. 59, preceding equation (1), and in his application of the term potential energy to E in the small print, designated by *energy* what is in reality exhaustion of energy or negative energy;

---

* See Clerk-Maxwell's article (p. 65) on "Capillary Attraction" ('Encyclopædia Britannica,' 9th edition).

and the same inadvertence renders the small print paragraph on p. 60 very obscure. The curious and interesting statement at the top of the second column of p. 63, regarding a drop of carbon disulphide in contact with a drop of water in a capillary tube would constitute a perpetual motion if it were true for a tube not first wetted with water through part of its bore—". . . if a drop of water and a drop of bisulphide of carbon be placed in contact in a horizontal capillary tube, the bisulphide of carbon will chase the water along the tube."

Additional Note of June 5, 1886.—I have carefully tried the experiment referred to in the preceding sentence, and have not found the alleged motion.

[W. T.]

WEEKLY EVENING MEETING,

Friday, May 14, 1886.

Henry Pollock, Esq. Treasurer and Vice-President, in the Chair.

Professor John Millar Thomson, F.C.S.

Suspended Crystallisation.

The phenomena attending the ordinary solution of metallic salts in water have been so often and ably brought before the consideration of this audience, that I have determined to confine myself this evening to certain considerations relating to the formation of and deposition from so-called supersaturated solutions of these salts. At the same time it is necessary for me to remind you of one or two points connected with the ordinary solution of a salt in water, which I may enumerate as follows.

The solubility of a salt in water depends:

(*a*) On the mass of salt presented to the water for solution, and the state of aggregation in which that mass may be at the time of solution.

(*b*) The temperature at which the solution of the salt is carried out; rise in temperature generally producing an increase in the solubility of the salt, although there are certain exceptions to this rule.

(*c*) Each salt has its own definite rate or amount of solubility; some being extremely soluble, as calcium chloride or sodium acetate; others having a very low amount of solubility, as calcium sulphate.

(*d*) On the cooling of a hot solution containing large quantities of salt, a deposition of the salt takes place, which deposition is known under the term "crystallisation."

Certain salts, however, which generally present abnormal phenomena in their solution show no tendency to be deposited from their solutions on cooling, provided such solutions are kept covered from the access of the outside air. Such solutions are said to have become supersaturated.

This branch of the subject is the one which will engage our attention.

It may be divided into two classes. (1) That which occurs in the presence of the undissolved salt; and (2) That which occurs in the absence of the undissolved salt; this latter class being the one we shall examine.

Glauber's salt (sulphate of soda) affords a very good example of this. If the crystallised sulphate be added to boiling water in a flask, as long as it is dissolved the water will take up nearly twice its weight of salt. If this solution be now allowed to cool in an open vessel an abundant deposition of crystals will take place, as the water when cold will only dissolve about one-third of its weight of crystallised sulphate. But if the flask be tightly corked or stoppered with cotton wool whilst the solution is boiling, it may be kept for several days without crystallising, although moved about from place to place. On withdrawing the plug, however, the air on entering the flask will produce a slight disturbance of the surface of the fluid, and from that point beautiful prismatic crystals shoot through the solution until the whole has become perfectly solid.

It is not necessary in preparing such solutions that the flask be closed completely with a stopper, as we find that plugging the neck with cotton wool exercises the same effect in preserving the solution, and that the air which enters the flask during the cooling becomes thoroughly deprived of its crystallising influence, evidently undergoing a filtering process. A very good instance of the inability of filtered air to start crystallisation is afforded by a solution of alum saturated at 194° F., and allowed to cool in a flask stoppered with cotton wool in the manner previously described. On withdrawing the plug of cotton wool the crystallisation, which in this case takes a much longer time to commence than with the sulphate of soda, will be seen beginning at various points on the surface of the liquid, and will spread slowly from these, octahedral crystals of alum half an inch or more in length being built up in a few seconds.

It is evident from these two experiments that the cause of this sudden crystallisation is to be sought for in the peculiar action of the air when it comes in contact with the solution; but that it is not due to the action of the air alone is shown by the fact that the air in the flask plugged with cotton wool must enter during the cooling, and that therefore the action of the cotton by filtering the air in some way deprives it of its active property.

But it will be found that there are other means of destroying this activity without filtering the air through cotton wool. Thus, if the solution of sodium sulphate containing two-thirds of its weight of crystallised salt be allowed to cool in a flask closed by a cork, furnished with two tubes plugged with cotton wool; on removal of the cotton plugs air may be blown from the lungs through the longer tube without causing the crystallisation, apparently from the fact that the air has been deprived of the nucleus which induces the crystallisation by passage through the lungs. On blowing air, however, from a bellows, after a few strokes the solution will be found to solidify almost instantaneously.

The earliest ideas concerning this sudden solidification were naturally those which supposed that the mere entrance of air into the flask upon opening it started the crystallisation. The late Dr. Graham, and Professor Thomas Thomson, held this view; and the former of these chemists carried out a considerable series of investigations on the action of different gases in determining the crystallisation. These ideas were followed by the statement of Ziz, that not only air but also solids were capable of acting as nuclei when dry, but that when wet, or boiled with the solution, or placed in it when hot and allowed to cool at the same time with it, they lost their effect. The activity of air, however, as in itself a nucleus, has been subsequently shown by Löwel to be incorrect; but at the same time he admits that solid bodies after exposure to air become active.

In 1851 two chemists, Selmi and Goskynski, introduced the explanation that dry air is active by virtue of its getting rid of the water at the surface, thus producing small crystals which continue the action. This seems to be a repetition of the theory propounded by Gay Lussac, who held that the air absorbed at the surface precipitated a portion of the salt in the same way that one salt precipitates another, and that this deposition continued the crystallising action. By an elaborate series of investigations, conducted in 1866, and also during later years, the French savants, MM. Gernez and Violette, came to the conclusion that there is only one nucleus for a supersaturated solution, and that is a crystal of the body itself; also indicating in certain experiments that substances which possess the same crystalline form and chemical structure may be found active to supersaturated solutions of each other. They have conclusively shown at the same time that heated or washed air, or bodies of different constitution but chemically clean, remain perfectly inactive as regards their supersaturated solutions.

That disruption of the solution may take place without causing crystallisation when the body added is perfectly clean may be easily shown by carefully removing the cotton plug from a solution of sodium sulphate containing only two-thirds its weight of crystals. On introducing a perfectly clean platinum wire no crystallisation is induced, but on removing the wire and touching it with a substance which has not previously been rendered chemically clean, and on again introducing it in the flask, the moment it touches the liquid crystallisation is at once induced.

This question of the nuclear action of substances upon these solutions has received considerable attention from English chemists, most notably from Mr. Tomlinson, Professor Liversidge, and Professor Grenfell, all of whom have conducted elaborate researches on the subject, leading, however, to different conclusions with each investigator. The arguments held by Tomlinson are strongly in support of some physical cause for the phenomenon, and that the result may be brought about by the action of substances possessing no chemical relations to the solutions experimented on. Professor Liversidge, on the other hand, has tried the action of several substances with the most careful precautions, that their addition to the solutions should be effected without contamination from, or access of, the external air to the flask during the experiments. The result of his inquiries, so far as the substances he has experimented with are concerned, confirms those of Gernez, and limits the number of bodies capable of acting as nuclei within very narrow limits. In fact, he concludes that the only body capable of causing the crystallisation of such solutions is a crystal of the substance itself, of exactly the same composition as it possesses when in a state of supersaturation. Thus Glauber's salt, which exists in a supersaturated solution, combined with 10 parts of water, will only crystallise by the addition of a crystal of the substance also containing 10 parts of water, and is perfectly inactive to crystals of the same salt which contain 7 proportions of water, or those which are anhydrous.

As it is almost impossible to conceive that our atmosphere is laden with minute particles of the many different metallic salts which we are acquainted with, some hesitation in accepting such a limited explanation may be excusable; but when we consider that it has been shown by Dr. Angus Smith and others that the air, especially in the vicinity of large manufacturing towns, is filled with small particles, more especially of Glauber's salt, we are not surprised that solutions of this body at least crystallise at once on the removal of the filtering medium. And the probability of the explanation may be further strengthened by the fact that these solutions when opened in the still air of country places may retain their liquid condition for considerable lengths of time.

At the same time the limit fixed by Professor Liversidge may appear a somewhat narrow one, and experiments made some time ago and published in the 'Journal of the Chemical Society of London' (May 1879, and September 1882), have confirmed a few experiments first indicated by Gernez, and go a considerable length in showing that bodies possessing not only the same crystalline form but an identical chemical structure are active nuclei in causing the crystallisation of supersaturated solutions.

In these experiments two methods for the addition of the nuclei intended to excite crystallisation were adopted. The first of these consisted of a flask and bulb tube, the supersaturated solution of the salt to be experimented on being placed in the flask, whilst the small bulb was filled with a solution of the body intended to act as nucleus. The solution in the bulb having been thoroughly boiled, the tube is stoppered with cotton wool and then introduced through the centre of a second cotton plug into the neck of the flask. The contents of the flask are now heated, the contents of the bulb receiving a second warming from the steam rising from the solution in the flask. The flasks so prepared are then allowed to stand till perfectly cold before performing an experiment. When it was desired to perform an experiment the solution of the body intended as nucleus contained in the bulb tube is first crystallised by touching with a platinum wire, or preferably by introducing a crystal of the salt contained in the bulb. Crystallisation having thus taken place in the bulb, and the crystal added having become enclosed in the fresh deposit, the bulb is lowered into the liquid contained in the flask and allowed to remain there for some time to show that the disturbance produced by its introduction into the fluid does not excite crystallisation. The bulb is finally lightly broken under the fluid and the result observed. Bulbs also containing water, pieces of washed glass, &c., may be broken under the supersaturated solutions, to show that the disruption produced on breaking does not excite crystallisation.

A second method which may be employed for the introduction of nuclei is that introduced by Professor Liversidge.* This consists of a siphon tube, in which crystallisation is induced in the first limb and allowed gradually to pass over the bend and down to the point of the second limb.

The following table gives the results of many experiments on the action of isomorphous and also of dissimilar substances on supersaturated solutions of each other.

ISOMORPHOUS SULPHATES ON MAGNESIUM SULPHATE.

Substance in solution.	Substance added.	Result.	Remarks.
$MgSO_4.7H_2O$	$ZnSO_4.7H_2O$	Active	Crystallisation induced at once, the crystals forming long needles.
„	$NiSO_4.7H_2O$	„	
„	$CoSO_4.7H_2O$	„	Crystallisation induced after some time, the crystals forming attached to the nucleus.
„	$FeSO_4.7H_2O$	„	
„	$MgSO_4.7H_2O$	„	
„	$NiSO_4.6H_2O$	„	Crystallisation induced after some time, the crystals attached to the nucleus generally being truncated needles.
„	$FeSO_4\,xH_2O$	„	
„	$CoSO_4.xH_2O$	„	

DISSIMILAR BODIES ON MAGNESIUM SULPHATE.

$MgSO_4.7H_2O$	$MgK_2(SO_4)_2.6H_2O$	Inactive.	
„	$Na_2SO_4.10H_2O$	„	
„	$Na_2S_2O_3.5H_2O$	„	
„	NaCl	„	
„	Glass	„	

ISOMORPHOUS SALTS ON SODIUM SULPHATE.

$Na_2SO_4.10H_2O$	$Na_2SeO_4.10H_2O$	Active	Crystallisation immediate.
„	$Na_2CrO_4.10H_2O$	„	„ „

---

* Proc. Roy. Soc., xx. 497.

Dissimilar Bodies on Sodium Sulphate.

Substance in solution.	Substance added.	Result.	Remarks.
$Na_2SO_4.10H_2O$ ..	$MgSO_4.7H_2O$ ..	Inactive.	
„	Na_2SeO_4	„	
„	$Na_2S_2O_3.5H_2O$ ..	„	
„	$Na_2HPO_4.10H_2O$ ..	„	
„	KCl	„	
„	NaCl	„	
„	$KClO_3$	„	
„	$NaI.4H_2O$	„	
„	Glass	„	

Chrome Alum and Iron Alum on Common Alum.

$AlK(SO_4)_2.12H_2O$ ..	$CrK(SO_4)_2.12H_2O$	Active ..	The chrome alum solution was prepared by saturating at 70°, and then allowing it to crystallise in the bulb tube.
„	$FeK(SO_4)_2.12H_2O$	„ ..	

Bodies of the same form or belonging to the same system, but not similarly constituted on Alum.

$AlK(SO_4)_2.12H_2O$ ..	NaCl (cubes) ..	Inactive.	
„	FeS_2 (cubes) ..	„	
„	Fe_3O_4 (octahedra)	„	

Hydro-disodic Phosphate and Hydro-disodic Arsenate.

$Na_2HPO_4.12H_2O$ ..	$Na_2HAsO_4.12H_2O$	Active ..	Crystallisation immediate and very rapid.

When two salts which are not isomorphous are in a supersaturated solution together a separation of one or other of the salts may be effected within certain limits. Thus in a mixture of sodium sulphate and nickel sulphate, the former may be crystallised by touching with a crystal of the salt, and in allowing the mixture to remain at rest for a few minutes the liquor containing the nickel sulphate may be entirely poured off from the crystals of sodium sulphate.

A similar phenomenon may be seen by preparing in a long glass cylinder two supersaturated solutions one above the other, the lower one being sodium thiosulphate dissolved in its water of crystallisation, the upper one sodium acetate dissolved in a quarter its weight of water. When the whole has cooled down under a stopper of cotton wool a crystal of sodium thiosulphate may be introduced; this will pass through the acetate solution without solidifying it, but will cause the immediate solidification of the thiosulphate solution.

The following phenomena may be seen in experiments carried out on mixtures of dissimilar salts.

A. When the mixture consists of two salts which are not isomorphous.

(1) Sudden crystallisation may take place, gradually spreading through the solution on the addition of a nucleus, causing a deposition of the body belonging to the nucleus only.

(2) That when sudden crystallisation takes place, causing the deposition of both salts, there is a preponderance of the salt of the same nature as the nucleus.

(3) That the nucleus may remain growing slowly in the solution, becoming increased by a deposition of the salt of the same nature as the nucleus.

B. When the mixture consists of two isomorphous salts.

(1) Sudden crystallisation may occur, giving a deposition of both salts, apparently in the proportions in which they exist in solution.

(2) That when slow crystallisation takes place, the nucleus increases by a deposition of the least soluble salt, showing that in mixed supersaturated solutions a gradation of phenomena may be experienced, passing from those shown in the crystallisation of a true supersaturated solution to those shown in the crystallisation of an ordinary saturated solution.

Passing from the action of nuclei on supersaturated solutions of mixtures of dissimilar salts, it is interesting to examine the action of the different constituents on supersaturated solutions of double salts. The following table gives the results of certain experiments with such double salts:—

Substance in solution.	Nucleus added.	Result.
$HgCl_2(NH_4Cl)_2,3H_2O$ ..	$HgCl_2$ (prismatic)	Active.
"	$HgCl_2$ (deposited from hot solution).	Both active and inactive.
"	NH_4Cl	Inactive.
$HgBr_2(NH_4Br)_2,3H_2O$ ..	$HgBr_2$ (deposited in the cold).	Active.
"	$HgBr_2$ (deposited from hot solution).	Both active and inactive.
"	$(NH_4)Br$	Inactive.
$HgI_2(KI)_2$	HgI_2 (needle-shaped crystals).	Active.
"	KI	Inactive.
$AlK(SO_4)_2,12H_2O$	$Al_2 3(SO_4),18H_2O$	"
"	K_2SO_4	"
$NaNH_4HAsO_4.4H_2O$ ..	$Na_2HAsO_4,12H_2O$	"
"	$(NH_4)_2HAsO_4,Aq_2$	"

From these experiments it will be seen that in the case of the double salts formed by the halogen acids, certain of the component salts are capable of inducing crystallisation, but in the case of the salts formed from acids of higher basicity the component salts are incapable of causing that particular disruption of the solution.

Having now treated this question experimentally, it may be of advantage to examine it shortly from a theoretical point of view to see if any explanation may be offered at least with our present knowledge of these sudden changes from liquid to solid.

At the present time it would be rash to attempt a complete answer to the questions—

(*a*) What fully takes place when a salt dissolves?

(*b*) Why some salts always separate out when their hot solutions are cooled: and conversely why certain ones remain dissolved under the same conditions?

In order that a salt may dissolve in water we must suppose some attraction between the molecules of the salt undergoing solution, and the molecules of the water. With most salts, the power of the water to dissolve them is increased with rise of temperature, and this rise in temperature means increase in the active movement of the water and the salt molecules, and therefore greater facility for them to come near enough to one another for their mutual attractions to be exerted.

Then why do not all salts dissolve more in hot than in cold water? all do not do so, as you have seen in my diagram during lecture. This leads us to following up the completion of this attraction; namely, the combination of the water and the salt molecules which I have alluded to in my lecture under the term hydration.

We must suppose that some hydrates exist at a higher temperature than others, and this is borne out by experiment. In the case of salts whose hydrates exist only at the lower temperatures, the effect of raising the temperature would be merely to increase the vibratory movements and so shake asunder the water and salt molecules, the dehydrated salt naturally separating out, and therefore we could not expect more to go into solution by merely heating the liquid.

With regard to the second point. We must remember that there exists a strong attraction between the individual molecules which compose the salt. Taking then the case of a salt-like potassium chlorate, which in the solid state contains no water attached to it. We dissolve it in hot water, and on cooling, much of the salt separates out. We can suppose that the attraction of the water for the salt and the active movement produced by the rise in temperature overcome this attraction of salt molecule for salt molecule, but as the solution cools, this exercises its full force, and crystallisation ensues.

Now, taking the instances where the salt remains in solution even after cooling, but in much larger quantities than can be obtained by treating the solid salt with water at that temperature; this being what I have called "suspended crystallisation."

Let us consider the case of sodium sulphate as perhaps the most familiar instance. You have seen a large volume of that salt suddenly solidify on the introduction of the proper nucleus. Now why did not that salt behave like the potassium chlorate instead of remaining in solution in the liquid after cooling?

It has been suggested that this *super*-saturated solution is merely a saturated solution of the anhydrous salt. That may or may not be so. If it be the case, we can suppose that the molecules of the salt and water are prevented from arranging themselves in their normal order and proportions, and so there is a kind of strain throughout the liquid. This can only be overcome by something which will disturb the molecules sufficiently to bring about the necessary rearrangement.

Taking the instance of the suspended solidification of water cooled below its freezing point; we know that only the disruptive effect of shaking is required; but with sodium sulphate in water and many others, no amount of shaking, as we have seen, is sufficient. Some stronger force is required; this stronger force is found so far as we know at present only in the attraction for the salt of the body itself, or of some substance having the same crystalline form and a similar chemical composition, as has been already shown.

I say advisedly at present, because in my opinion it has not yet been conclusively proved, that no form of what we should call simply mechanical disturbance may not bring about sudden crystallisation in these so-called supersaturated solutions. Indeed, there is an interesting experiment which seems to foreshadow such a possibility. By dropping a single carefully washed crystal of alum into a supersaturated solution of that salt, we notice a very interesting phenomenon. The whole surface of the solution is covered with small crystals separated by definite and considerable intervals, and it appears as if the mere mechanical disturbance produced by the first crystal attracting to itself similar molecules, had caused the union of other molecules to form crystals in the remoter parts of the liquid. This is, I think, a very interesting case, which if studied with other similar instances may throw additional light on the causes of such crystallisation. If we suppose that the salt exists in solution as a hydrate, that is, in actual combination with water, we can imagine that each individual molecule of the hydrate attracts each other one, and is attracted by it equally; so if one molecule were to move towards another, it would be restrained by its neighbour, and that in its turn by those near it, and so a state of equilibrium would come about. However, whatever may be the condition of the salt in solution, the same cause, namely, the attraction of similar molecules, appears always to render the equilibrium unstable.

[J. M. T.]

WEEKLY EVENING MEETING,

Friday, May 21, 1886.

The Duke of Northumberland, K.G. D.C.L. LL.D. President, in the Chair.

Sir John Lubbock, Bart. M.P. D.C.L. LL.D. F.R.S. *M.R.I.*

The Forms of Seedlings: the causes to which they are due.

Sir John Lubbock commenced the lecture with some general remarks on the innumerable types of foliage among mature plants and the causes to which we might refer their various forms, the breadth of some and narrowness of others, the differences of position, the differences of length in conifers, &c. He said that these considerations had led him to study the cotyledons or first leaves of seedlings. Cotyledons do not present such extreme differences as leaves; nevertheless, they afford a very wide range. Some are broad, some narrow, some are long, some short, some are stalked, some sessile, some lobed, some even bifid or trifid. At first sight, these differences seem interminable, and it might appear hopeless to attempt to explain them. Sir John Lubbock, however, pointed out, as regards many species, taking especially the commonest plants, such as the familiar mustard and cress, the beech, sycamore, pink, chickweed, &c., the conditions of their formation and growth, and it is beautiful to see the various reasons to which the differences are due, gradually unfolding themselves; the same result being sometimes brought about by very different circumstances, emargination of the cotyledons, for instance, being due to at least six different causes. He mentioned one curious peculiarity in the seedling of a species allied to our common mistletoe. It is a parasitic species, and its fruit, like that of the mistletoe, is somewhat viscid, so that it adheres to any plant on which it falls. But, even if it reaches the plant on which it grows, it may light on an unsuitable position—say, for instance, a leaf. What then happens? The radicle elongates for about an inch and then developes on its tip a flattened disc, which applies itself to the plant. If the situation be suitable, there it grows; if not, the radicle straightens itself, tears the berry from the spot where it is lying, curves itself, and then brings the berry down on to a new spot. The radicle then detaches itself, curves in its turn, and thus finds a new point of attachment. We are assured that this has been seen to happen several times in succession, and that the young plant thus seems enabled to select a suitable situation.

The form of the cotyledons, or seedleaves, depends greatly on that of the seeds, long narrow seeds naturally in most instances producing embryos with narrow cotyledons. The cases, however, which can be so simply accounted for are comparatively few. Many plants with narrow cotyledons have flattened and orbicular seeds. In such species, however, the cotyledons lie transversely to the seed. An interesting case is afforded by the pink family, where the pink itself has broad cotyledons, while the chickweed has narrow ones. In both cases the seeds are flattened and orbicular, but in the pink the seed is dorsally compressed, and the cotyledons lie in the broad axis of the seed; while in the chickweed seed is laterally flattened, and the cotyledons lie transversely to the seed.

Another very interesting case which he gave is that of the genus Galium, to which the common "cleavers" of our hedges belongs. Here also we find some species with narrow, some with broad cotyledons; but the contrast seems to be due to a very different cause. Galium aparine has broad, Galium saccharatum narrow, cotyledons. So far as the form of the seed is concerned there is no reason why the cotyledons should not be much broader than they are. The explanation may perhaps be found in the structure of the pericarp, which is thick, tough, and corky. It is very impervious to water, and may be advantageous to the embryo by resisting the attacks of drought and of insects, and perhaps even if the seed be swallowed by a bird, by protecting it from being digested. It does not split open and is too tough to be torn by the embryo. The cotyledons therefore, if they had widened as they might otherwise have done, would have found it impossible to emerge from the seed. They evade the difficulty, however, by remaining narrow. On the other hand, in Galium aparine the pericarp is much thinner and the embryo is able to tear it open. In this case therefore the cotyledons can safely widen without endangering their exit from the seed. The thick corky covering of Galium saccharatum is doubtless much more impervious to water than the comparatively thin test of Galium aparine. The latter species is a native of our own Isles, while Galium saccharatum inhabits Algiers, the hotter parts of France, &c. May not then perhaps, he suggested, the thick corky envelope be adapted to enable it to withstand the heat and drought. In this genus, as in many other plants, the embryo occupies only a part of the seed, being surrounded by a store of food or "perisperm." In many cases the embryo occupies the whole seed, and the cotyledons must, therefore, in large seeds, either be thrown into various folds, as in the beech, or be thick and fleshy as in the bean or oak. The reasons for their numerous differences open up an inexhaustible variety of interesting questions. Sir John gave a great number of examples, which were rendered clearer by means of numerous diagrams of seeds and seedlings.

In conclusion, he said it might be asked whether the embryo conformed to the seed, or the seed to the embryo, and showed that, at least as regards certain species, the former was the case; while the shape of the seed, again, might be shown to be influenced by considerations connected with the construction of the fruit. In reply to this, he compared the seedlings of the sycamore and of the oak. In the sycamore, the seed is more or less an oblate spheroid, and the cotyledons, which are long and ribbon-like, being rolled up into a ball fit it closely, the inner cotyledon being generally somewhat shorter than the others. On the other hand, the nuts of the beech are triangular. An arrangement like that of the sycamore would therefore be utterly unsuitable, as it would necessarily leave great gaps. The cotyledons, however, are folded up somewhat like a fan, but with more complication, and in such a manner that they fit beautifully into the triangular nut. Can we however, he said, carry the argument one stage further? Why should the seed of the sycamore be globular, and that of the beech triangular? Is it clear that the cotyledons are constituted so as to suit the seed? May it not be that it is the seed which is adapted to the cotyledons? In answer to this, we must examine the fruit, and we shall find that in both cases the cavity of the fruit is approximately spherical. That of the sycamore, however, is comparatively small, and contains one seed, which more or less exactly conforms to the cavity in which it lies. In the beech, on the contrary, the fruit is at least twice the diameter, and contains from two to four nuts, which consequently, in order to occupy the space, are compelled (to give a familiar illustration, like the pips of an orange) to take a more or less triangular form. Thus then, he said in conclusion, in these cases, starting with the form of the fruit, we see that it governs that of the seed, and that the seed again determines that of the cotyledons. But, though the cotyledons often follow the form of the seed, this is not invariably the case. Other circumstances, as I have attempted to show, must also be taken into consideration, and we can throw much light on the varied forms which seedlings assume.

I fear you may consider that I have occupied your time by a multiplicity of details, and I wish I could hope to have made those little plants half as interesting to you as they have made themselves to me; but, at any rate, I may plead that without minute, careful, and loving study, we cannot hope in science to arrive at a safe and satisfactory generalisation.

The lecture was accompanied not only by numerous diagrams, but by specimens, kindly lent by the authorities of Kew, and by some practical illustrations.

WEEKLY EVENING MEETING,

Friday, May 28, 1886.

Sir Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Professor Oliver Lodge, D.Sc.

The Electrical Deposition of Dust and Smoke, with special reference to the collection of metallic fume, and to a possible purification of the atmosphere.

The research of which I have the honour to speak in this theatre to-night, takes its origin in an observation made by Dr. Tyndall, frequently shown by him in this place, and of which he has published an account in your 'Proceedings' for 1870.

The phenomenon discovered by Dr. Tyndall was a dark or dust-free space formed over a hot body when held in strongly illuminated dusty air. A flame or hot poker held beneath the concentrated beam of an electric lantern shows it perfectly, and the appearance is as of wreaths of black smoke. [Experiment.]

That it is not smoke, but anti-smoke, is perhaps rendered most evident by observing the utterly different appearance of smouldering brown paper held in the same place. [Experiment.] In fact it is manifest that what we see in a sunbeam is not the beam itself but the dust illumined by it. It is somewhat misleading, though customary, to speak of the motes as rendering a sunbeam visible; it is really the beam which renders visible the motes. And the more motes there are, the more there is to see, naturally; hence smoke in the beam greatly increases its luminosity, while anything which removes or expels the dust-particles destroys its luminosity, leaving a clear dark channel.

Now what is the cause of this dust-free space? The first idea was that the dust is burned and destroyed by the flame. But this explanation is negatived by using a moderately warm body, say a hot kettle, and noticing that above this also the dark space is perfectly distinct, though narrow.

Dr. Frankland next suggested that much of visible dust consisted of moisture, and that it was dried and rendered invisible by warmth. Both explanations can be at once disproved by replacing common miscellaneous dust of unknown origin by a carefully chosen smoke, say that of burnt magnesium, which is certainly neither volatile nor combustible.

Finally Dr. Tyndall suggested an ingenious mechanical explanation, that the air was dragged up in convection currents faster than its supported dust, which consequently got left behind; and with this he was content.

So the matter rested until 1881, when Lord Rayleigh re-examined the phenomenon, "not feeling satisfied with the explanation given by the discoverer," and conclusively established the inefficiency not only of all ready-made hypotheses concerning it, but also of those which occurred to himself. A most interesting reversal of the experiment was made by Lord Rayleigh, for, instead of holding a warm body under the beam, he held a cold body above it, and witnessed the formation of a down-streaming dark plane of great sharpness, bordered by bright fringes of extra dusty air, which he attributed to the dew-point being passed and the consequent condensation of moisture upon dust-nuclei.

Three years ago I took up the subject at Liverpool, and, with the active co-operation of the late Mr. J. W. Clark, then acting as Demonstrator, repeated all known experiments with minute care, in order to find out the cause of the singular phenomenon.

The results of this research are numerous; I can do no more than summarise a few of them to-night, though I believe that some of them, if followed up, would lead to results of considerable interest. The first thing we observed was that the ascending dark plane was the extension and upstreaming of a dust-free *coat* which invested the surface of the warm body, and which was really the essential part of phenomenon, the dark plane being merely an upstreaming extension of it. From gently warm bodies the upstreaming dark plane, being made by the uniting of two coats, one from either side of the body, is easier to see than the coats themselves, but they are usually quite distinct, and on the average may be taken as about the hundredth of an inch thick.

[Appearance of coat and plane shown by diagram.]

Round red-hot bodies the coat is obtrusively evident. We first observed the coat on the concave side of a hemi-cylinder of sheet copper. We tried this shape in order to disprove Lord Rayleigh's suggested explanation that the dust-freeness might be due to the curvature of the stream-lines and centrifugal force. This idea it does disprove most effectively, for the curvature being negative on the inside of the cylinder, dust ought to be thrown towards the body, instead of being kept well away from it, as in fact it is.

[Lantern slide of planes.]

It is not easy to project the actual dark coat and plane on to a screen, because the light from the dust particles is but feeble; but it is possible to send a beam *along* the rod, instead of across, and to show that the dust-free space is more transparent than the surrounding smoky air. In this way a projection of a thick coat and plane, in light, upon the screen, is easily made.

[Experiment. A platinum wire a few inches long is stretched across a glass box, and arranged to be pulled straight at all temperatures. A parallel beam is sent exactly along the wire, and through a lens, the box is filled with brown paper smoke, and a few Grove cells applied.]

In trying this experiment one often finds a copy of the appearance imprinted upon the glass wall of the box in smoke, and this dust-graph also serves to illustrate the phenomenon.

[Projection on screen of dust-graph formed on a vertical glass plate, where the end of a hot rod is held against it in still dusty air.]

It may be convenient here to state, at least roughly, the line of explanation to which we have ultimately been impelled. We regard it as quite certain that the facts serve as another illustration of the kinetic theory of gases, of the same kind as Mr. Crookes's radiometer. The dust particles are kept out of contact with the warm body by means of a differential molecular bombardment on their surfaces. True, the thickness of the coat is far larger than the mean free path of a molecule at the corresponding pressure; and accordingly no body as big as a radiometer vane could at that distance be acted upon; but Prof. Osborne Reynolds has shown that very minute bodies are susceptible to a bombardment at much greater distances; and it is, I think, in accord with his theory that dust particles should be repelled from hot bodies over a measurable distance even at ordinary atmospheric pressure, although radiometer vanes would only be acted upon at a pressure of a thousandth, or something approaching a millionth, of an atmosphere. I do not say that this explanation is yet complete, and I am not aware that any one has adopted it. It is, however, pretty clear that there will be an outward pressure from the surface of a hot body,[*] so long as the air near it is getting warmer: and if convection currents are allowed, the air near a hot body is *continually* in the state of getting warmer.

The main facts which we assured ourselves of may be thus briefly summarised.

1. The dark plane becomes visible over a cylindrical thermometer bulb indicating a temperature only half a degree warmer than the air.
2. The dark coat becomes just visible round a body one degree hotter than the air; at two degrees it is distinct; and at five degrees it is fairly thick.
3. The coat enlarges with diminished pressure, and narrows when the pressure is increased.
4. In hydrogen the coat is thicker, and in carbonic acid thinner, than in air.
5. If volatile smoke like volatilised camphor is used, the coats become thicker, though less sharply defined.
6. Round a rod of camphor in ordinary smoke the dust-free coat is extra thick, owing to the extra bombardment of evaporation.
7. Round cold bodies the coat is practically absent, especially on the top of the body; and if the cold is too great no descending dark plane is formed: only a bright one.

---

[*] See for instance some experiments of Mr. Crookes on repulsion by hot surfaces, described in 'Nature,' vol. xv. page 301.

8. *Liquids* containing solid powder in suspension (e. g. dried ferric oxide in water) form a very narrow dark plane over a moderately warm cylinder. But the thickness of the plane is so slight that it is difficult to observe, and it gets thinner with increase of temperature, instead of thicker as in the case of gases; hence the experimental rod in a liquid must be only gently warmed.

The formation of a descending dark plane in dusty air below a moderately cool body is singular, for it would not naturally have been expected on the bombardment hypothesis. The dust particles must be driven towards, instead of away from, a cold body; and this fact is obtrusively evident, from the fact that such a body becomes quickly covered with soot in an atmosphere of MgO or other smoke, while a warm body remains clear.

It is easy to illustrate this fact by two black conical flasks, one full of hot water, the other of cold, both put under a bell-jar of thick white smoke. In about five minutes, if they be taken out, it will be found that the hot one is nearly free from soot, the cold one covered as with hoar-frost.

[Experiment. Either because they were left too long, or for some other reason which I proceed to investigate, this experiment in this particular instance completely failed.]

Mr. Aitken, of Edinburgh, has also observed this fact, and has pointed out that it explains the deposition of soot in chimneys, and of lampblack on cold glass. Mr. Aitken's paper in the 'Transactions of the Royal Society of Edinburgh' for 1884, is a most comprehensive and lucid account of all this part of the subject. His work was concurrent with ours, as related in the 'Philosophical Magazine' for 1884, and it is better described.

Bodies colder than the air in contact with them have the dust in it bombarded on to them; as is well seen on a wall above hot-water pipes, or on a ceiling above a gas-jet. Smoking of the gas-jet will of course provide more material to be deposited, but the dust and smoke naturally (or unnaturally) in the air are usually ample to effect a sufficient blackening, over even a perfectly clear flame. An incandescent electric lamp hung a foot or so under a white ceiling will similarly cause a small black patch.

In rooms warmed by radiation (open fire or sunlight) objects are warmer than the air and keep much dust off themselves; though the bombardment may not be sufficiently vigorous to overcome the gravitation of the larger dust particles, especially over a flat horizontal surface, where convection is sluggish.

In stove-heated rooms things are liable to be colder than the air, and thus get exceedingly dusty.

The cause of the clearing of smoky air inside a bell-jar by the introduction of a hot platinum wire, or other hot body, as observed by Tyndall, who called it calcining of the dust, is now manifest: the dust is bombarded on to the sides and floor of the vessel by the warmed air. The self-formed picture of dark coat and plane formed on a glass surface when the end of a hot cylinder is held against it is similarly completely explicable.

But, it may be asked, if dust gets driven towards cold bodies instead of away from them, how is it that any dark or dust-free plane forms beneath them? At very low temperatures I believe it does not, but rather a bright or dusty layer forms instead. From a rod a few degrees below the air a fine dark plane is visible, however, edged by two bright ones. I do not know what Lord Rayleigh's view of this descending plane is, but it may be due to the gravitative settling of the dust through the air immediately beneath the cold body: a thing which is shown to occur by the existence of a thin dark half-coat formed underneath, but not above, such a rod, and by the deposit of dust on its upper surface. The rod shelters the air immediately beneath it from the shower, and so a layer of clear air forms and streams downward continuously. If the downward convection currents are too rapid the settling has not time to occur, and no dark plane is visible.

The original plan of experiment included a series of measurements of the thickness of the dark-coat at different temperatures (both excess and absolute), at different pressures, and in different kinds of gas. This research Mr. Clark had indeed begun to arrange for, but his untimely death last year cut short this part of the investigation. Another very necessary thing is to see how it varies with size of dust particles. Meanwhile I had devoted myself to developing a branch of the subject which we had accidentally hit upon in the course of testing one hypothesis which had at an early stage occurred to us as perhaps a clue to the cause of the phenomenon of the dust-free plane. We thought it possible that air in streaming over the surface of the solid might get electrified, and that, from air so electrified, dust might somehow be expelled. To test this hypothesis, we purposely electrified the rod, positively and negatively, to see what happened. A hundred volts or two produced a barely noticeable effect; positive electrification causing a slight widening, negative electrification, a slight narrowing, of the dust-free coat. But as soon as the potential rose to a few thousand volts, and brush discharge began to be possible, a very violent and remarkable effect was noticed: the dark coat widened enormously and tumultuously, and the whole box was rapidly cleared of smoke.

To specially observe this new phenomenon is very easy. Fill a bell-jar with any kind of smoke: tobacco, camphor, turpentine, magnesia, ammonic chloride, ammonic sulphite, brown paper, steam, phosphoric oxide, lead fume, zinc fume, no matter what, and then discharge electricity into it from a point connected with a Voss or Wimshurst machine, or even with a common frictional machine, the other pole of which is connected with the ground or with the base of the bell-jar.

In a second or two, aggregation of the smoke particles sets in, they form in masses or flakes along the lines of force, and in another instant the jar is clear of smoke: it has all been condensed on the sides and floor of the vessel.

[Experiment.] *

The kind of smoke used is quite immaterial, it is all acted upon in the same way, but to make the effect visible to an audience, it is better to use something which does not dirty or render opaque the glass. Burning magnesium ribbon makes a very good and clean smoke. For experiments on a large scale a cheap smoke is obtained by burning sulphur in the neighbourhood of a pan of ammonia. Whether positive or negative electricity be used seems to make no difference.

Instead of a single point, a double set of points may be used, each connected with one pole of the machine. A round knob will act instead of a point, but not so quickly. Brush discharge, or anything that electrifies the air itself, is the most effectual. When a knob or pair of knobs is used, the lines of force are interestingly mapped out by the dust-flakes.

The cause of the phenomenon is manifest enough. The electrified or polarised particles attract each other, and are attracted by the opposite poles, just as iron filings are influenced near a magnet. In thinking over what manifestations of this aggregating power of electricity were already known, the beautiful observations of Lord Rayleigh on water-jets occurred to me; though the cause in this case is not so clear. The experiment is not so well known as it should be, and, being an extremely simple one, I venture now to show it. A vertical water-jet two feet high, from an opening $\frac{1}{20}$ inch in diameter, scatters into drops and falls as a shower like rain; but hold a piece of rubbed sealing wax a yard or so distant from the place where the jet breaks into drops, and they at once cease to scatter: they fall in large blobs as a thunder-shower.

[Experiment. The rain may be allowed to patter on to paper, when the difference in sound is very distinct. The air should be free from electricity beforehand, as the jet is extremely sensitive.]

Clouds can probably be caused to rain by discharging electricity into them; at any rate, a cloud of steam in a bell-jar rapidly turns into Scotch mist or fine rain, and so disappears.

[Experiment. Insulation in this case is a slight difficulty, but it is easily managed.]

To make a thick mist or fog it is sufficient to introduce a scrap of burning sulphur under the bell-jar; instantly the country mist becomes more like a town fog, but this also is rapidly dispersed by electricity.

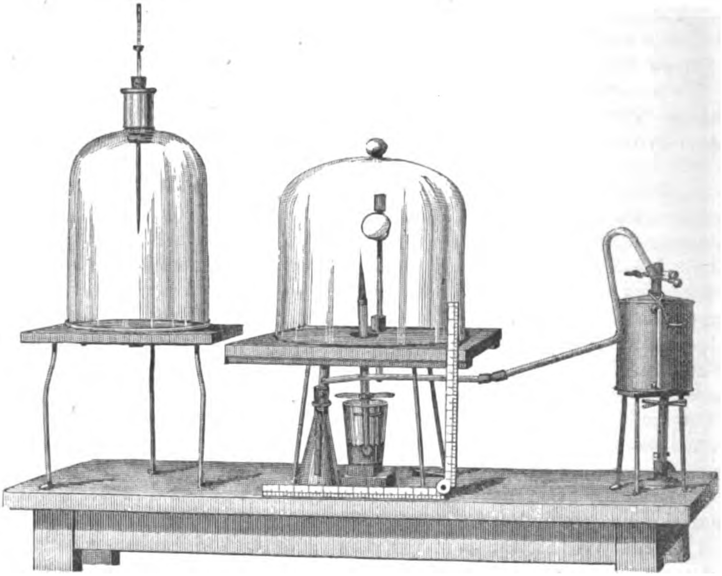
[Experiment.]

I have ventured to think it possible that the coagulation or combination of oppositely charged dust particles is a gross imitation of

---

* *Note added July* 1886. My attention has just been drawn to a paragraph in 'The Mechanics' Magazine' for November 1850, wherein it appears that this phenomenon was at that time observed by a Mr. C. F. Guitard; a fact of which I had been quite ignorant.

the cohering of oppositely charged atoms in a chemical compound. When nitrogen and hydrogen are subjected to sparks they unite gradually into ammonia. When a brush discharge passes into oxygen it aggregates into ozone, as is well known from the smell near an electrical machine in action. Such actions (especially the latter) do seem to me of the same nature as the aggregation of charged smoke particles, though far more refined. The size of a smoke particle is not great, but it is enormous compared with an atom. Each granule of a lycopodium cloud may be taken as containing pretty exactly one trillion molecules. A smoke particle is a good deal smaller than a lycopodium granule, but not incomparably smaller. It is because of the minuteness and the interleaved arrangement of oppositely charged atoms in a compound, that its electrical capacity is so enormous; so that ten billion units of each kind of electricity can be stowed away among the atoms of a milligram of water, without raising the potential of each above a volt or two.*



Two bell-jars as used for smoke and fog condensation in lecture experiments. The one with the point let in at top is for any dry smoke; the other is for any damp or acid smoke. Insulation is in this latter case quite outside the chamber, and the rod, swathed in glass or gutta-percha, enters through a wide hole in the base-board. The little flask is only to catch drip. The ivory ball is to be illuminated by a beam of light, and to shine through the fog as it clears.

---

* In connexion with this part of the subject, see B.A. Report for 1885 (Aberdeen), page 744, where an electrostatic theory of chemistry is discussed.

Application of the Coagulating Power of Electricity to practical purposes.

In many industries the presence of fine dust or fume suspended in the atmosphere is highly objectionable, sometimes because of the poisonous or dangerous nature of the dust, sometimes because it is valuable and apt to escape and be wasted.

In flour-mills and coal-mines, the fine dust is dangerously explosive. In lead, copper, and arsenic works it is both poisonous and valuable.

Mr. Alfred Walker, of Walker, Parker & Co., first informed me of the difficulty which lead-smelters labour under in condensing the fume which escapes along with the smoke from red-lead smelting furnaces, and he wished to put an electrical process of condensation to the test on a large scale.

The devices which are in use at different works to collect or condense this fume are very numerous, and some of them very cumbersome. Prof. Roberts-Austen has been kind enough to lend me diagrams illustrating several of these contrivances.

At Bagillt the method used is a large flue two miles long, coiled up in the side of a hill between the furnace and the chimney; much is retained in this flue, but still a visible cloud of white lead fume continually escapes from the top of the chimney.

The only difficulty in the way of depositing fume in the flue by means of a sufficient discharge of electricity, is the violent draught which is liable to exist there, and which would blow away mechanically any deposited dust. In some ways the blast may be helpful, for instance, by keeping the electrical points clear and preventing local clogging, but a large chamber must be provided somewhere for the coagulated flakes of dust to settle and remain in calm.

The plan I suggest at present is to line a certain portion of the interior of a flue with spikes, and then to hang in the middle of it wire netting, well insulated and studded all over with ragged edges and points. It may be suspended lengthways in the flue. If the chamber be very large, it may be well to have a number of long prickly nets arranged parallel to each other, and kept alternately positive and negative.

The insulation of the suspended conductor, whatever its shape, has of course most carefully to be arranged: everything depends upon that.

[Experiment showing the action of an electrical point held above a model chimney emitting smoke. Its first effect is to destroy or reverse the draught: but if the chimney be itself provided with points at the top, and an electrified cap be held over it, e. g. a small sheet of prickly wire gauze, the draught is assisted, and the smoke is mainly condensed.]

I do not regard this as a good method of dealing with smoky chimneys: the right way to deal with them is to abolish them, i. e. to make combustion so perfect that no unburnt matter escapes.

The experiment illustrates, however, that smoke can be caught and deposited on the wing.

[Special apparatus for producing and condensing quantities of smoke was next shown in action. It could be kept electrified by a splendid machine kindly lent by Mr. Wimshurst, but a small Voss machine was amply sufficient for the purpose. After the lecture I had an opportunity of trying Mr. Wimshurst's 8-plate machine on the smoke-chamber, and the rapidity with which it was cleared was surprising. Roughly speaking it might be called instantaneous.]

When such a machine as this of Mr. Wimshurst's comes to be used, one may hope to make an impression on fume produced on a manufacturing scale. But further data in this direction are desirable.

Leaving these sublunary and industrial applications, let us ask finally if there may not perhaps be some possible mode of artificially affecting atmospheric conditions by means of electricity. It certainly seems to me rather probable; and I should much like to have the means of discharging a large quantity of high-pressure electricity into the air, either for the purpose of acting on a fog, or in the hope of perhaps affecting the weather.

This much I regard as certain, that if a kite or a captive balloon (a kite for windy days, a balloon for calm ones) be flown into a cloud, and made to give off electricity for some time, that cloud will begin to rain.

It is just possible, though hardly probable, that by the automatic coalescence of drops into larger ones, the potential of the charge so given could become high enough to cause an artificial thunderstorm.

The amount of electricity in a thunderstorm is not very great (Faraday reckoned it as less than the amount in a thimble-full of water, and he was quite right); its potential is enormous, but there is certainly some automatic regenerative action going on in the atmosphere, which is able to raise the potential of a given charge higher and higher until a flash occurs.

It is well known, from observations made with Sir W. Thomson's atmospheric recorder, that variations in the electrical condition of the air precede a change of weather, but it is not known which is cause and which is effect in this case.

With a very large voltaic battery such as that constructed by Dr. de la Rue, or perhaps even a bigger one, of sufficient E.M.F. to give a constant brush discharge from points, a tremendous quantity of electricity could be poured into the atmosphere, and its electrical condition could be certainly disturbed.

Whether this disturbance would be beneficial or not, is, I know, another matter. I do not see how one is to tell without trying the experiment.

Perhaps usually the weather is best left alone; but there are times when any change feels as if it must be for the better.

Very little is known at present concerning the causes operating in the formation and dissipation of those Cyclones which act so powerfully on our climatic conditions as to be now the main object of study with meteorologists, since Mr. Buchan proved to us their existence and showed us how to study them. Some of them are gigantic, but some are small and almost local whirls. A long way we are at present from even entertaining the idea of artificially controlling them; but we have learnt to control a great number of natural agents—torrents, cataracts, fire, electricity; and the main effect of a thunderstorm on a civilised town is now merely to clear its air and stir up its lightning conductors. Storms, earthquakes, and volcanoes we have not yet learnt to control, perhaps we never shall. Perhaps also we may always remain unable to exert any effect even on ordinary winds, clouds, and rain; but the interests involved at certain seasons of the year in the presence or absence of a succession of rain-compelling cyclones, or of frost- and sun-transmitting anti-cyclones, are so prodigious, that even the barest glimpse of the possibility of exerting any kind of effect upon these great agents is worth attention, and it is a subject eminently suited for experiment. The experiments, however, must be on a large scale, and must be costly; were it not so, I for one should not hesitate to make some preliminary attempts in the direction indicated.

References to the Literature of the Subject.

Proc. Roy. Inst. vi. p. 1 (1870), Tyndall (Discovery of dust-free space).
Proc. Roy. Soc. xxv. p. 542, Frankland (Brief).
"Floating Matter of the Air," Tyndall, p. 5.
Proc. Roy. Soc. December 1882, or Nature, xviii. p. 139, Lord Rayleigh (Dark plane).
Nature, 26th July, 1883, xxviii. p. 297, Lodge (Preliminary Letter).
Nature, 31st January, 1884, Aitken (Abstract).
Phil. Mag. March, 1884, or Proc. Phys. Soc. February 1884, Lodge and Clark (Main paper, with plate).
Trans. R. S. Edin. xxxii. p. 239 (1884), Aitken (Main paper, with plate).
MINOR PAPERS:
    Nature, 24th April, 1884, Lodge (Lecture to Royal Dublin Society, on Dust-free spaces).
    Nature, 22nd January, 1885, Lodge (Lecture to Brit. Assoc. Montreal, on Dust).
    Nature, xxix. p. 417, Lodge and Clark (Abstract).
    Engineering, 5th June, 1885, Walker.
    Engineering, 21st May, 1886, Wimshurst.
    La Nature, or Electrician, 21st May, 1886, Tissandier.
Nature, xv. p. 302 (1877), Crookes (Repulsion by hot Surface).
Phil. Trans. 1879, ii. p. 727, Osborne Reynolds (Dimensional Properties of Gases).
Proc. Roy. Soc. March and May, 1879, and June 1882, Lord Rayleigh (Liquid jets).
Brit. Assoc. Report, 1885, pp. 744 et seq. Lodge (Electrostatic view of Chemical Action).

[O. L.]

WEEKLY EVENING MEETING,

Friday, June 4, 1886.

John Rae, M.D. LL.D. F.R.S. Manager, in the Chair.

Walter H. Gaskell, M.D. M.A. F.R.S.

The Sympathetic Nervous System.

The lecturer commenced by giving a short sketch of Bichat's views of the division of life into organic and animal life, and pointed out how that division naturally led to the conception of two separate central nervous systems, the one, the sympathetic, to which all the organic functions are to be referred, the other, the cerebro-spinal, regulating the animal functions. He then pointed out how Remak's discovery of a special kind of nerve-fibre,—the non-medullated nerves—associated only with the ganglia of the sympathetic system, tended strongly to confirm Bichat's teaching of the existence of two separate central nervous systems in the human body, each of which communicated with the other by means of its own special kind of nerve-fibres; the cerebro-spinal supplying the sympathetic system with white medullated fibres, and the sympathetic supplying the cerebro-spinal with grey or gelatinous non-medullated fibres. He then continued as follows:—

Even at the present day the teaching of Bichat still very largely holds its ground. It is true that the tendency of modern physiology is to increase the number of centres of action for the organic nerves, which exist in the cerebro-spinal central axis, and therefore to do away with the necessity for a separate independent sympathetic nervous system, yet the automatic actions of isolated organs such as the heart, and the existence of special nerve-fibres in connection with this system still induce the neurologists of the present day to place the sympathetic nervous system on an equality with the brain or spinal cord. In this lecture to-night I hope to give the death-blow to Bichat's teaching, and to prove to you that the whole sympathetic system is nothing more than an outflow of visceral nerves from certain nerve-centres in the cerebro-spinal system, the ganglia of which are not confined to one fixed position as is the case with the ganglia of the posterior roots, but have travelled further away from the central axis.

I do not propose to-night to deal with the argument for the independence of the sympathetic nervous system, which is based upon the automatism of such isolated organs as the heart; I have already in various papers given the reasons and arguments why I look upon such automatic movements as due to the automatism of the cardiac muscular tissue rather than to any action of nerve-cells comparable to the nerve-centres of the spinal cord; I shall deal entirely with the anatomical argument and show you step by step how the nerve-fibres which constitute the sympathetic system can be traced to their origin in the central cerebro-spinal axis.

Evidently, in endeavouring to determine by anatomical means whether the sympathetic and cerebro-spinal systems are in reality independent of one another, our attention must necessarily be especially concentrated upon the nature of the connecting link between the two systems, i.e. upon the nature of the rami communicantes. Largely owing to the preconceived notions of anatomists, you will find that the rami communicantes are arranged symmetrically in connection with all the spinal nerves of the body. In reality this is far from being the case, the rami communicantes of the thoracic nerves differ from those above them, i.e. of the cervical nerves, and from those below them, i.e. of the lumbar nerves, in two important particulars: in the first place the corresponding sympathetic ganglion is connected with each thoracic nerve by two rami communicantes; and secondly, these two rami differ in colour, one being grey, i.e. composed almost entirely of non-medullated nerves, and the other white, i.e. composed essentially of medullated nerve-fibres.

This double nature of the ramus communicans is confined to the region lying between the two large plexuses which supply the anterior and posterior extremities, viz. the brachial, lumbar, and sciatic plexuses; the rami communicantes to the lower cervical and first thoracic nerves, as well as those to the nerves forming the anterior crural and the sciatic, are, on the other hand, single, and are composed only of grey rami. In other words, the sympathetic chain is connected with the central nervous system by means of white rami communicantes only between the second thoracic and second lumbar nerves.

Further, I have been able to trace both the white and grey rami in their journey to the spinal cord by means of consecutive sections of osmic acid preparations, and have found that the grey rami pass out of the sympathetic ganglion as a single nerve, and then ramify in the connective tissue about the vertebral foramina, a portion only reaching the spinal nerve-trunk; the grey fibres of this portion pass mainly along the nerve peripherally, the few which pass centrally never reach the spinal cord, but pass out with the connective tissue which lies in between the medullated nerve-fibres of the anterior and posterior roots, to ramify over and to supply the blood-vessels of the various membranes which inclose the spinal cord.

In fact the grey rami communicantes are peripheral nerves, which partly supply the vertebræ and the membranes of the cord, and partly pass to their destination in the same direction as the efferent fibres of the spinal nerve itself.

So far then I come to these conclusions:

1. The sympathetic does not send non-medullated fibres into the

cerebro-spinal system, because these fibres all pass out of the nerve-roots before they reach the spinal cord.

2. White or medullated nerve-fibres constitute the only link between the sympathetic and cerebro-spinal systems, constituting the white rami communicantes.

3. Consequently the connection between these two nervous systems is limited to the region of white rami communicantes, i. e. to the region between the second thoracic and second lumbar nerves.

Further, these conclusions are borne out when we attempt to follow the white rami communicantes into the central spinal axis by means of their structural peculiarities; sections of osmic preparations show that each white ramus is composed chiefly of very small medullated nerve-fibres, varying in size from $1 \cdot 8\ \mu$ to $3 \cdot 6\ \mu$, very much smaller, therefore, than the large medullated nerves which form the bulk of the anterior roots of the spinal nerves, these latter varying between $14\ \mu$ to $20\ \mu$ or even larger. Clearly then the fibres of the white ramus communicans ought to show very conspicuously among the large fibres of the anterior roots whenever they are present in those roots. I have cut sections of the anterior roots of all the spinal nerves in the dog, and have found, as I show you on this screen, that these very fine medullated nerve-fibres make their appearance for the first time in the anterior roots of the second thoracic nerve; they are found in large quantities in all the anterior roots between the second thoracic and second lumbar, and then again the anterior roots immediately below the second lumbar are free from such groups of very fine fibres. We see then that exactly corresponding to the presence of white rami communicantes in the thoracic region we find groups of characteristic fine medullated fibres existing in the anterior roots, fibres which clearly form part of the white ramus communicans, and confirm by their presence the conclusion already arrived at, viz. that the nerves which pass from the spinal cord into the sympathetic system are limited to the thoracic region of the cord.

We can now go a step further and argue in the reverse direction that the presence of groups of these very fine medullated fibres in the anterior roots of any nerve implies the existence of nerve-fibres belonging to the same system as the white rami communicantes or rami viscerales as we may now call them. Examination shows how just is this argument, for I find that the same groups of fine nerve-fibres suddenly appear again in the anterior roots of the second and third sacral nerves, and can be traced into that well-known nerve which passes from the second and third sacral nerves into the hypogastric plexus to supply the rectum, bladder, and reproductive organs; a nerve, therefore, which may be looked upon as the white ramus communicans of the sympathetic ganglia which form the hypogastric plexus.

Again, in the cervical region, although such groups of fine fibres are absent from the anterior roots of all the cervical nerves, yet they form a conspicuous part of the upper roots of the spinal accessory nerve, and upon tracing them outwards I find that they separate entirely from the large fibres of the accessory which form its external branch, to pass as the internal branch into the ganglion trunci vagi (see Fig. 2, p. 536). Here, then, we see in the upper cervical region that the internal branch of the spinal accessory nerve is formed on the same plan as a white ramus communicans, the ganglion belonging to which is the ganglion trunci vagi.

Among the cranial nerves we find, especially in the vagus, glosso-pharyngeal, and chorda tympani, groups of fine nerve-fibres belonging to the same system. We can therefore say that the communication between the so-called sympathetic and cerebro-spinal systems is not symmetrical throughout, but consists of three distinct outflows of characteristic visceral nerves, viz.: 1, cervico-cranial; 2, thoracic; 3, sacral, the break of continuity corresponding to the exit of the nerve plexuses which supply the upper and lower extremities.

These medullated visceral nerves then pass out from the central nervous system into the various ganglia of the sympathetic, and it is possible that these latter ganglia bear the same kind of relation to them as the ganglia on the posterior roots bear to the sensory nerves. Before, however, we can accept this view it is absolutely necessary to account for the non-medullated nerves which arise from the sympathetic ganglia. Now it is hopeless to follow, by anatomical means, any special nerve-fibre through the confusion of a ganglion. What we cannot effect by anatomical methods we can by physiological. If we find two nerves, one of which enters a ganglion and the other leaves it, and we find their function absolutely the same on both sides of the ganglion, we have a perfect right to conclude that we are dealing with the same nerve in different parts of its course. Thus, in the case of the posterior root ganglion, the same sensory nerves are found on each side of the ganglion, although they are in connection with nerve-cells of the ganglion itself.

So also with the sympathetic ganglia; we know, for instance, that the nerves which increase the rate and strength of the heart's beat, pass to the ganglion stellatum along the rami communicantes of the second and following thoracic nerves, and we know also that the same nerves pass to the heart from the ganglion stellatum, from the annulus of Vieussens, and from the inferior cervical ganglion. Now seeing that these nerves are known to pass out of the cord in anterior roots, and from thence into the white rami communicantes of the upper thoracic nerves it follows that they are medullated in this part of their course, and are to be found among the bundles of very fine medullated nerves which we have seen are characteristic of the anterior roots of this region and of the white rami communicantes.

We can then say with certainty that the accelerator nerves enter the ganglia stellata as fine white medullated nerves. I am also able to say with absolute certainty that the accelerator nerves in that part of their course which lies between the chain of sympathetic ganglia and the heart are entirely composed of non-medullated fibres.

I know no other bundle of nerve-fibres which is so absolutely free from medullated nerves: in other words, nerve-fibres of the same function enter a sympathetic ganglion as white medullated fibres and leave it in increased numbers as grey non-medullated nerves.

Throughout we find the same fact, all the vasomotor nerves behave in exactly the same manner as the accelerators of the heart. In all cases the non-medullated fibres of the sympathetic are simply the fine medullated visceral nerves which have passed from the spinal cord in one or other of the three visceral outflows and lost their medullary sheath in their passage through the ganglia of the sympathetic system; together with that loss of medulla they have increased in number by division.

Seeing then that the non-medullated (so-called sympathetic) nerve-fibres are throughout modified medullated (so-called cerebro-spinal) fibres, and do not, therefore, arise in the sympathetic ganglia, we may fairly look upon the sympathetic ganglia as bearing the same kind of relation to the visceral nerves that the ganglia of the posterior roots bear to the ordinary sensory nerves. This conception is remarkably confirmed by the observations of Onodi, who has shown that the ganglia of the sympathetic are developed in close connection with the posterior root ganglia, and travel further away from the central axis as the animal grows.

Finally, the meaning of the sympathetic as a simple outflow of ganglionated visceral nerves from certain portions of the spinal cord and medulla oblongata is to my mind conclusively settled by the intimate relationship which exists between the structure of the spinal cord and the presence or absence of rami viscerales. In the grey matter of the spinal cord we find, as shown in the accompanying diagram, certain well-defined groups of nerve-cells, viz. *a*, a group of large nerve-cells in the anterior horn (4 in Fig.); these are known to be the origin of ordinary motor-fibres (4); *b*, a group of nerve-cells (3) split off from this and forming the lateral horn; *c*, a group (2) known as Clarke's column; and *d* and *e*, two sets of nerve-cells (4 and 5) in the posterior horn connected with sensory nerves. All these groups of nerve-cells are found along the whole length of the spinal cord, except those of Clarke's column. Their connection with nerve-fibres of different functions is known, except those of Clarke's column. Thus both sets in the anterior horn are connected with ordinary motor nerves; both sets in the posterior horn with ordinary sensory nerves. Now Clarke's column is limited to certain definite regions of the cord, being conspicuous: firstly, between the second thoracic and second lumbar nerves; secondly, at the top of the cervical region and extending into the cranial region; and thirdly, an isolated patch in the sacral region. In other words, its cells correspond exactly in position to the distribution of the white rami communicantes, so that, corresponding to the variation of this cell-group we find variations of the number of very fine medullated fibres in the anterior roots, and we find corresponding variations in the white rami communicantes, which latter, as I have told you, are the only true connections of the cerebro-spinal nerve-centre with the sympathetic. In other words, we have driven home to their origin these visceral nerve-fibres, and we find that they do not arise from any nerve-cells outside the brain and spinal cord, but from a definite nerve-group within the spinal cord.

FIG. 1.

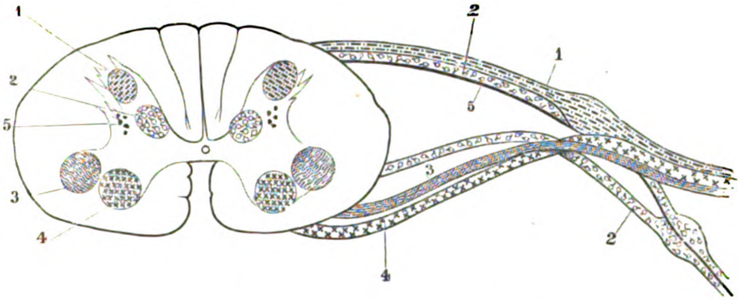
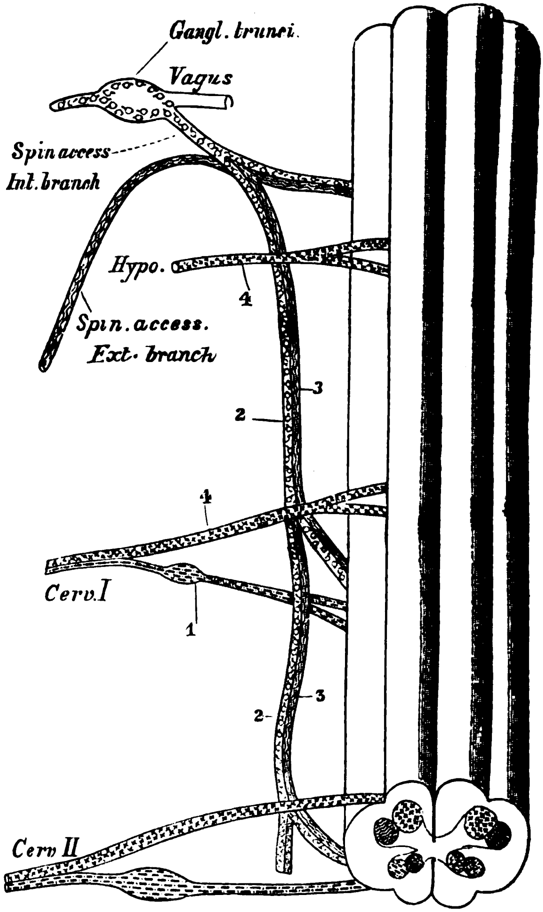


Fig. 1.—Diagram of section of spinal cord to show the various groups of nerve-cells in the grey matter, and the formation of a spinal nerve with its sympathetic ganglion.

1. Cells of posterior horn and somatic sensory nerves.
2. Cells of Clarke's column and ganglionated splanchnic nerves.
3. Cells of lateral horn and non-ganglionated splanchnic nerves.
4. Cells of anterior horn and somatic motor nerves.
5. Solitary cells of posterior horn and splanchnic sensory nerves.

We can, I think, go further than this, and say, with Bichat, that two nerve-systems do exist, the one for organic, and the other for animal life. These two, however, are not separate and distinct, but form parts of the same central nervous system. Looking at this diagram of the upper cervical region of the cord, we see that the voluntary striped muscles may be divided into two groups, according to their nerve supply, viz. a group supplied by the anterior (4), and one by the lateral horn of nerve-cells (3), and we know also that these two groups of nerve-cells separate from one another more and more as we pass into the brain region. So that we find for the muscles of the face a distinct separation of two groups, viz. 1, those which move the eyes and the tongue—these are supplied by nerves which arise from the continuation of the anterior horns; and, 2, the muscles of expression and mastication, the nerves of which arise from the continuation of the lateral horn; and remembering how the smile, the laugh, and the snarl, as well as the action of swallowing, are at the bottom only modified respiratory movements, we see that Charles Bell was not so far wrong when he inserted a lateral or respiratory system of nerves in between the anterior and posterior roots. This insertion is actually to be seen at the upper part of the cervical cord (see Fig. 2), where a separate nerve is formed by elements which arise laterally, known as the spinal accessory, and what is most striking is this fact, that in this region the fine medullated fibres (2 in Fig.) are found only in connection with these lateral motor nerves, and not with

FIG. 2.


Gangl. trunci.
Vagus
Spin access
Int. branch
Hypo.
4
Spin. access.
Ext. branch
3
2
4
Cerv I
1
3
2
Cerv II

the anterior motor, so that not only do these lateral or respiratory tracts supply special muscles with motor nerves, but these motor nerves have a closer relationship to the visceral nerves than other motor nerves. What is true of the upper cervical region is true also of the medulla oblongata. Here again, the visceral fine medullated nerves are closely connected with the motor fibres which arise from the lateral horn, e. g. the chorda tympani and the facial. Undoubtedly this particular group of muscles has some closer relationship to the viscera than other trunk muscles, and that relationship is explained immediately if we can accept and extend van Wijhe's investigations, viz. that in the cranial region the muscles which are supplied by the third, fourth, sixth, and twelfth cranial nerves are derived from the myotomes, while the muscles supplied by the seventh and fifth cranial nerves are derived from the lateral plates of mesoblast.

In fact, we may look upon the body as composed of two parts—an outside or somatic part, and an inside or splanchnic part. Each part has its own system of voluntary muscles; each part is supplied by nerves arranged on the same plan, viz. a ganglionated and non-ganglionated portion; and each part has its own individual centres of action, the inside portion of the grey matter of the spinal cord containing the centres for the splanchnic roots (2, 3, 5, in Fig.), i. e. the centres of organic life; the outlying horns the centres for the somatic roots (1 and 4), i. e. centres for the animal life. It is a strange and suggestive fact that these two sets of centres are not arranged symmetrically along the spinal axis, but that two great breaks occur in which the centres of organic life fall into the background in comparison to those of animal life. These two great breaks correspond to the origin of the nerves for the legs and arms, and suggest that the formation of the limbs in the originally symmetrical ancestor of the vertebrata—i. e. the large outgrowth of somatic elements in two definite portions of the body, caused of necessity a corresponding increase in the centres for animal life, while there was no necessity for a corresponding increase in the centres for organic life. The oldest part of us is undoubtedly the vital part; those organs and their nervous system by which the mere act of existence is carried on. With these two there may have been originally a symmetrical segmental arrangement of locomotor organs. Such symmetry, however, went for good when it was found more convenient to concentrate the locomotor machinery into the anterior and posterior extremities and with the asymmetrical arrangement of the locomotor organs disappeared also the symmetry of the central nervous system. This correspondence between the plan of the central nervous system and the development of the extremities is to my mind strongly in favour of the view which I have put before you to-night. In conclusion, I thank you for the kindness with which you have listened to me, and hope that I have succeeded in convincing you that Bichat's teaching of an independent sympathetic system is finally dead.

[W. H. G.]

GENERAL MONTHLY MEETING,

Monday, June 7, 1886.

Sir Frederick Abel, C.B. D.C.L. F.R.S. Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:—

Sir Frederick Abel, C.B. D.C.L. F.R.S.
Sir William Bowman, Bart. LL.D. F.R.S.
Right Hon. The Lord Halsbury.
William Huggins, Esq. D.C.L. LL.D. F.R.S.
Sir John Lubbock, Bart. M.P. D.C.L. LL.D. F.R.S.
Sir Frederick Pollock, Bart. M.A.
Henry Pollock, Esq. Treasurer.
Sir Frederick Bramwell, F.R.S. Honorary Secretary.

Sir Nathaniel Barnaby, K.C.B.
Charles Selby Bigge, Esq. J.P.

were elected Members of the Royal Institution.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India. Records, Vol. XIX. Part 2. 8vo. 1886.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quarta, Vol. VI. No. 3. 8vo. And Disegni. fol. 1886.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. Vol. II. Fasc. 8, 9. 8vo. 1886.
Academy of Natural Sciences, Philadelphia—Proceedings, 1885, Part 3. 8vo. 1886.
Agricultural Society of England, Royal—Journal, Second Series, Vol. XXII. Part 1. 8vo. 1886.
Ashburner, C. A. Esq. (the Author)—Geology of Natural Gas in Pennsylvania. 8vo. 1885.
Product and Exhaustion of the Oil Regions of Pennsylvania and New York. 8vo. 1885.
Asiatic Society, Royal—Journal, Vol. XVIII. Part 2. 8vo. 1886.
Astronomical Society, Royal—Monthly Notices, Vol. XLVI. No. 6. 8vo. 1886.
Bankers, Institute of—Journal, Vol. VII. Part 5. 8vo. 1886.
Bell, Sir Lowthian, Bart. F.R.S. M.R.I. (the Author)—Iron Trade of the United Kingdom. 8vo. 1886.
Birt, W. Esq.—Holiday Notes in East Anglia. 12mo. 1886.
British Architects, Royal Institute of—Proceedings, 1885–6, No. 16. 4to.

Chemical Society—Journal for May, 1886. 8vo.

Domville, Wm. H. Esq. M.R.I.—Natural History Journal of the Hungarian National Museum, 1882. 8vo. 1883.

East India Association—Journal, Vol. XVIII. Nos. 2, 3. 8vo. 1886.

Editors—Amateur Photographer for May, 1886. 4to.
American Journal of Science for May, 1886. 8vo.
Analyst for May, 1886. 8vo.
Athenæum for May, 1886. 4to.
Chemical News for May, 1886. 4to.
Engineer for May, 1886. fol.
Horological Journal for May, 1886. 8vo.
Industries, Part 1. fol. 1886.
Iron for May, 1886. 4to.
Nature for May, 1886. 4to.
Revue Scientifique for May, 1886. 4to.
Telegraphic Journal for May, 1886. 8vo.
Zoophilist for May, 1886. 4to.

Franklin Institute—Journal, No. 725. 8vo. 1886.

Geographical Society, Royal—Proceedings, New Series, Vol. VIII. No. 5. 8vo. 1886.

Geological Society—Quarterly Journal, No. 166. 8vo. 1886.

Gordon, Surgeon-General C. A. M.D. C.B. M.R.I. (the Author)—New Theory and Old Practice in Medicine, &c. 8vo. 1886.

Hambleton, G. W. Esq. (the Author)—What is Consumption? 8vo. 1886.

Harlem Société Hollandaise des Sciences—Archives Néerlandaises, Tome XX. Liv. 4. 8vo. 1886.

Historical Society, Royal—Transactions, New Series, Vol. III. Part 2. 8vo. 1886.

Johns Hopkins University—Studies in Historical and Political Science, Fourth Series, No. 5. 8vo. 1886.
University Circular, No. 48. 4to. 1886.
American Chemical Journal, Vol. VIII. No. 2. 8vo. 1886.

Lawrence-Hamilton, J. Esq. M.R.I.—Allgemeines Handbuch der Freimaurerei. 3 vol. 8vo. 1863–7.
Francmaçonnerie. Par J. M. Ragon. 6 vol. 8vo.
Œuvres Maçonniques de N. C. Des Etangs. Par F. D. Pillot. 8vo. 1848.
Manuel Maçonniques. 8vo. 1820.
Cours Oral de Franc-Maçonnerie Symbolique. Par H. Cauchois. 8vo. 1863.
Collection des Cahiers des Grades Symboliques. 4to. 1858.
Explication des Douze Ecussons. Par F. Bouilly. 4to. 1838.
Études Historiques et Symboliques sur la Franc-Maçonnerie. Par F. Vaillant. 12mo. 1860.
Recueil Précieux de la Maçonnerie. 16mo. 1786.
Le Secret de la Société des Mopses. 16mo. 1745.
Katechismen der Freimaurerei. Von R. Fischer. 3 vol. 12mo. 1873–5.
Masonic Ritual and Monitor. By M. C. Duncan. 12mo. 1866.
Manual of Freemasonry. By R. Carlile. 12mo.
Freemason's Monitor. By T. S. Webb. 12mo. 1868.
Les Rituels Maçonniques avec Commentaires. Par J. Lawrence-Hamilton. 8vo. 1873.
Catalogue of Books on Freemasonry, &c. 8vo. 1870.

Linnean Society—Journal, Nos. 143-4, 150. 8vo. 1886.

Madden, T. More, Esq. (the Author)—Memorials of Dr. R. R. Madden. 8vo. 1886.

Manchester Geological Society—Transactions, Vol. XVIII. Parts 17, 18. 8vo. 1886.

Meteorological Office—Monthly Weather Report for Dec. 1885 and Jan. 1886. 4to.

Meteorological Society, Royal—Quarterly Journal, No. 58. 8vo. 1886.
Meteorological Record, No. 20. 8vo. 1886.

National Association for Social Science—Conference on Temperance Legislation, London, 1886. 8vo.
National Life-boat Institution, Royal—Annual Report, 1886. 8vo.
Odontological Society of Great Britain—Transactions, Vol. XVIII. No. 7. New Series. 8vo. 1886.
Pharmaceutical Society of Great Britain—Journal, May, 1886. 8vo.
Photographic Society—Journal, New Series, Vol. X. No. 7. 8vo. 1886.
Robins, E. C. Esq. (the Author)—The Site of the new Admiralty and War Offices. 8vo. 1886.
Saxon Society of Sciences, Royal—Mathematische-Physische Classe: Abhandlungen, Band XIII. No. 5. 8vo. 1886.
Second Geological Survey of Pennsylvania—Reports A A and Atlas, C 5 and T 3. 8vo. 1885.
Smith, Basil Woodd, Esq. M.R.I.—Middlesex County Records. Vol. I. Edited by J. C. Jeaffreson. 8vo. 1886.
Society of Arts—Journal, May, 1886. 8vo.
St. Pétersbourg, Académie des Sciences—Bulletins, Tome XXXI. No. 1. 4to. 1886.
Tasmania Royal Society—Proceedings for 1885. 8vo. 1886.
United States Geological Survey—Bulletins, Nos. 15-23. 8vo. 1885.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1886: Heft 4. 4to.
Victoria Institute—Journal, No. 77. 8vo. 1886.

WEEKLY EVENING MEETING,

Friday, June 11, 1886.

HENRY POLLOCK, Esq. Treasurer and Vice-President, in the Chair.

PROFESSOR DEWAR, M.A. F.R.S. *M.R.I.*

Recent Researches on Meteorites.

PROFESSOR DEWAR explained that Mr. Gerrard Ansdell and himself had been engaged in an examination into the gaseous constituents of meteorites, and he proposed this evening to abstract the results of the investigation, which had been laid before the Royal Society.

The nature of the occluded gases which are present to a greater or less extent in all meteorites, whether belonging to the iron, stony, or carbonaceous classes, has engaged the attention of but few chemists. It is, nevertheless, an especially interesting and important subject, owing to the uncertainty which still exists as to the origin of these celestial bodies.

Graham (Proc. Roy. Soc. vol. xv. (1867) p. 502) was the first who made any experiments in this direction, when he determined the gases occluded in the Lenarto meteoric iron, which yielded 2·85 times its volume of gas, 86 per cent. of which was hydrogen, and 4·5 per cent. carbonic oxide. He was followed in 1872 by Wöhler (Pogg. Ann. vol. cxlvi. p. 297) and Berthelot (Compt. Rend. vol. lxxiv. pp. 48, 119), who estimated approximately the gases contained in the Greenland Ovifak iron. These gases consisted of about equal parts of carbonic acid and carbonic oxide; the celestial origin of this iron is, however, very doubtful.

In the same year (1872) the American chemist, Mallet (Proc. Roy. Soc. vol. xx. p. 365), made a very complete determination of the gases occluded by the Augusta Co., Virginia, meteoric iron, which, however, differed very considerably from Graham's results. He obtained an amount of gas equal to 3·17 times the volume of the iron, made up of 35·83 per cent. of hydrogen, 38·33 per cent. carbonic oxide, 9·75 per cent. of carbonic acid, and 16·09 per cent. of nitrogen.

Wright and Lawrence Smith followed Mallet, and our present knowledge of this interesting subject is principally due to these American chemists. They have taken advantage of the numerous meteoric masses which have fallen from time to time throughout America, and which can easily be obtained in sufficient quantity for complete and accurate observations on their gaseous constituents.

Wright contributed several papers to the 'American Journal' in 1875 and 1876, and, according to his analyses, the total volume of gas occluded and the composition of the same differs considerably in the two principal classes of meteorites. He found the total volume of gas extracted was much greater in the case of the stony meteorites than in the iron ones, the principal characteristics of these gases being, that in the former the carbonic acid greatly predominated, accompanied by a comparatively small amount of carbonic oxide and hydrogen, whereas in the latter the carbonic acid never exceeded 20 per cent., the carbonic oxide being, as a rule, considerably more than this, and the hydrogen sometimes reaching as high as 80 per cent.

It is impossible, however, to arrive at anything more than general conclusions as to the total amount of gas given off by any special meteorite, or its composition, for, as shown by Wright and confirmed by Mr. Ansdell and the lecturer, both the total quantity and composition of the gases vary very considerably according to the temperature at which they are drawn off.

Wright found a notable quantity of marsh-gas in all the stony meteorites which he examined, though not a trace in any of the iron ones; this seemed to be a distinctive difference between the two classes of meteorites, but subsequently Dr. Flight, of the British Museum (Phil. Trans. vol. clxxiii. (1882) p. 885), found marsh-gas in a specimen of the Cranbourne siderite, so that it is evident certain of the iron meteorites also contain this gas.

Lawrence Smith (Amer. Journ. 1876) confined himself principally to an examination of the graphite nodules which are frequently found imbedded in the iron meteorites, and to the nature of the carbon in the so-called carbonaceous meteorites. He extracted the organic or hydrocarbon-like bodies by means of ether, but did not determine the gases given off on heating. Previous to this, Roscoe (Pro. Phil. Soc. Man. 1862) had obtained the same hydrocarbon-like body by exhausting the Alais meteorite with ether, but the quantity he had to work upon was so small that he could not make a very complete examination.

These are some of the principal points that have been made out with regard to the gases occluded by meteorites. The results, however, are so comparatively few, that it was thought worth while to take the opportunity which presented itself, of having several good specimens of meteorites to confirm these results, and, if possible, add something to our present knowledge of the subject.

The investigation may be divided into five parts, having the following objects in view: firstly, the confirmation of previous results by the examination of some well-known meteorite; secondly, the analysis of several whole meteoric stones, whose interior had never been exposed to the effects of the atmosphere, by reason of the characteristic coating of glaze; thirdly, the examination of a celestial graphite nodule, taken from the interior of an iron meteorite; fourthly, the comparison of some meteorite of the carbonaceous class with the above; and fifthly, the examination of different terrestrial graphites.

The method employed for the abstraction of the gases was exactly the same in every case, so that a short description will suffice for all. The temperature was kept as nearly as possible the same in every experiment, but no doubt differences of many degrees occurred in some of the experiments, which was unavoidable in using an ordinary combustion furnace.

The meteorite or graphite, as the case might be, was broken up into a coarse powder, introduced into a convenient length of combustion tubing, and connected up with a Sprengel pump, a small bulb-tube immersed in a freezing mixture intervening, so as to retain any moisture or condensable volatile products that might come off. The tube was first thoroughly exhausted and then heated in an ordinary gas combustion furnace to a low red heat. The gases, during the heating, were gradually drawn off by the Sprengel pump, and when the tube had remained for several minutes at a low red heat it was completely exhausted. The total quantity of gas collected was in every case used for the analysis.

The "Dhurmsala" specimen was an ordinary fragment of a much larger original mass, but in the case of the Pultusk and Mocs meteorites, complete stones were fortunately obtained, weighing respectively 57 and 103 grams, having the characteristic black glaze on their surfaces.

Such a large quantity of water was condensed in the bulb tube in heating the Dhurmsala meteorite, it being the first one examined, that it was thought it might be principally due to the great absorptive power of these porous bodies, and that therefore the moisture might have been condensed in the pores of the meteorite from the surrounding air. The Pultusk and Mocs specimens appeared to be especially adapted for ascertaining whether this was the case, as the complete covering of black glaze would probably prevent the moisture from penetrating to the interior of the stones. The fragments of these stones were therefore transferred as quickly as possible to the combustion tube after they had been broken up. Notwithstanding these precautions, fully as much water was condensed from them as from the Dhurmsala specimen, which seems to suggest that the water is really combined in some form in the stone and not obtained directly from the surrounding atmosphere, although it must be admitted that the glaze on both the stones was not of a very glossy character, and did not have the appearance of being absolutely impervious to moisture.

The pumice-stone was examined merely with a view to comparing the gases occluded by a porous body of volcanic origin with those contained in meteorites. The sample taken was a fresh piece of stone, which had not been dried or purified in any way.

It is evident that it differs considerably from the meteoric stones, the total occluded gas being very small, only about half its volume, the carbonic acid at the same time being much less, with a proportionate increase in the carbonic oxide.

The general method of analysis was as follows, and the accuracy of the results was confirmed by varying in some cases the method of separating the gases. The carbonic acid was first removed from the mixture by caustic potash, the carbonic oxide being then absorbed by subchloride of copper, and the remainder of the gases exploded with excess of oxygen. The carbonic acid formed was again removed by caustic potash, and the excess of oxygen by alkaline pyrogallate, the residue being taken as nitrogen. The relative quantities of marsh-gas and hydrogen were calculated from the total diminution after explosion, and the amount of carbonic acid formed:—

	Sp. Gr.	Occluded Gases in vols. of the Meteorite.	Percentage Composition.				
			CO_2.	CO.	H.	CH_4.	N.
Dhurmsala	3·175	2·51	63·15	1·31	28·48	3·9	1·31
Pultusk	3·718	3·54	66·12	5·40	18·14	7·65	2·69
Mocs	3·67	1·94	64·50	3·90	22·94	4·41	3·67
Pumice-stone ..	2·50	0·55	39·50	18·50	25·4	..	16·60

It will be seen that the above numbers are quite confirmatory of Wright's results, the carbonic acid in the three meteorites examined being by far the largest constituent, while marsh-gas in considerable quantity was found in all. The percentage of this latter gas is somewhat higher than that found by Wright in the stony meteorites he examined, but this is probably due to the fact that a rather higher temperature was employed by the lecturer to drive off the gases. This supposition seems to be confirmed on considering the analysis of the Pultusk meteorite; for whereas Wright's abstracted gas only reached 1·75 times the volume of the stone, the total quantity of gas obtained by the lecturer was twice as much or equal to 3·54 times its volume.

It is therefore unquestionable that marsh-gas is given off on heating these meteoric stones, but whether it exists as such occluded in the material, or whether it is formed by some chemical decomposition of some organic constituent of the mass, is by no means clear.

Wright came to the conclusion "that the marsh-gas really existed as such in the stony meteorites, as the temperature at which it was driven off would be too low for its formation," at the same time he thinks it quite possible that "at very much higher temperatures, in the reaction by which the carbonic acid is broken up by the iron, a portion of the carbon might combine with the hydrogen present to form marsh-gas."

Knowing the great absorptive power for gases possessed by porous bodies generally, it was thought advisable to determine directly what this absorptive power was in the case of these stony meteorites, which are of such an eminently porous nature.

For this purpose the powdered Dhurmsala meteorite, from which the gases had been removed, was left in moist air under a bell-glass, for different periods of time as tabulated below, the gases being drawn off at a low red heat after each period:—

	Occluded Gas in vols. of the Meteorite.	CO_2.	CO.	H.	N.
After 24 hours	0·61	54·0	..	42·4	3·6
After 6 days more	2·47	47·0	5·0	47·0	1·0
After 8 days more	0·63	96 1	2·0	1·5	..

The absorption of water and gases evidently went on tolerably rapidly for the first seven days, but after the second heating of the meteorite, its porosity seems to have been affected in some way, for after a further period of eight days, it was found to take up only about a fourth of the quantity of gas which it had absorbed in the previous six days.

The actual amount of water given off after this exposure to a moist atmosphere was considerably less than what was obtained in the original heating of the meteorite, and from this it was inferred that the water is chemically combined in the stone. It would be difficult to explain, otherwise than by chemical combination, the power by which the water is retained by these meteorites, as it is not given off until a very high temperature is reached. In any case it is clear that the hydrogen must come from the action of water on the iron-nickel alloy, or finely disseminated carbon. Greville Williams has pointed out that the large amount of hydrogen obtained from heating finely divided zinc-dust is not due to free hydrogen, but to the action of the zinc on the hydrated oxide of zinc.

To pass on to the consideration of the various graphites examined. The celestial graphite was a perfect oblong nodule weighing 30 grams, which had been taken from the interior of a mass of the Toluca meteoric iron. It had a uniform dull-black colour, except at one end where there was a slight incrustation of sulphide of iron. Its fracture showed a uniform dull-black, compact mass; it was easily pounded up in a porcelain mortar, and formed a fine granular powder without any lustre.

On extracting the gases from this specimen a considerable quantity of marsh-gas was obtained, so that it appeared most important to compare it with some samples of terrestrial graphites, more especially as the occluded gases had never, as far as the lecturer was aware, been determined in these bodies.

For this purpose four samples of native graphites were obtained. The Cumberland graphite was a magnificent specimen of the original Borrodale, and had been in a private cabinet for over fifteen years. It had the characteristic dense homogeneous structure and brilliant external lustre of the graphites coming from this district. The Siberian example was from the Alexandref Mine; its structure was columnar and striated, with little external lustre; it was rather more easily broken up than the Borrodale, but formed the same dull black powder. The specimen from Ceylon was of the type usual to that island: highly lustrous and flaky, breaking up very easily, and forming small shining plates when ground up. The last sample, which was from the same cabinet as the others, but whose origin was unfortunately unknown, had a dull external surface, was exceedingly porous, and much more brittle than any of the previous ones, grinding up very easily into a dull black powder. It had more the appearance of the celestial graphites, which was heightened by having slight incrustations of sulphide of iron on its surface. Its low specific gravity also shows it to be some exceptional variety.

It seemed most important in connection with this subject to examine some matrix with which the graphites are usually found associated. These rocks are very variable, but consist principally of a kind of decomposed trap or gneiss. A good specimen was obtained of semi-decomposed gneiss from Canada with a considerable quantity of graphite disseminated throughout the mass, and also several samples of Ceylon graphite imbedded in its matrix, which in this case consisted of felspar and quartz.

The results, as tabulated below, confirm Wright's analyses of several trap rocks, in which he found principally carbonic acid and hydrogen. The small quantity of marsh-gas no doubt comes from the disseminated graphite, but the presence of the hydrogen is more difficult to explain and requires further investigation.

	Sp. Gr.	Occluded Gases in vols. of the Graphite.	CO_2.	CO.	H.	CH_4.	N.
Celestial graphite ..	2·26	7·25	91·81	..	2·50	5·40	0·1
Borrodale „ ..	2·86	2·60	36·40	7·77	22·2	26·11	6·66
Siberian „ ..	2·05	2·55	57·41	6·16	10·25	20·83	4·16
Ceylon „ ..	2·25	0·22	66·60	14·80	7·40	3·70	4·50
Unknown „ ..	1·64	7·26	50·79	3·16	2·50	39·53	3·49
Gneiss „ ..	2·45	5·32	82·38	2·38	13·61	0·47	1·20
Felspar	2·59	1·27	94·72	0·81	2·21	0·61	1·40

On comparing these samples of graphite, it will be seen that the Borrodale and the Siberian give off about the same total volume of gas, that the celestial and the unknown graphites closely approximate each other in this respect, yielding more than double the volume of the others, and that the Ceylon sample stands alone in yielding a very minute quantity. All the terrestrial samples, except that from Ceylon, are alike in giving off a very considerable quantity of marsh-gas, though they differ somewhat in the actual quantity, and it is evident that although the celestial graphite contains a considerable amount, it is very much less than that yielded by the terrestrial samples.

A few tentative experiments were made to ascertain the absorbing power for gases of this celestial graphite. For this purpose dry carbonic acid, marsh-gas, and hydrogen were respectively drawn through the tube containing the previously exhausted graphite for twelve hours in the cold, the gases being pumped out at a low red heat after each treatment with the dry gas. After the carbonic acid treatment the volume of gas collected was only 1·1 times that of the graphite, containing 98·4 per cent. of carbonic acid; after the marsh-gas the volume of the gas was only 0·9 that of the graphite, containing 94·1 per cent. carbonic acid; and after the hydrogen the volume of the gas collected was only 0·17 times that of the graphite, containing 95·0 per cent. of carbonic acid. It is therefore evident that the large quantity of gas occluded in celestial graphites cannot be explained by any special absorptive power of this variety of carbon. In view of the large and varying percentages of marsh-gas in the gaseous products of all these graphites, it appeared of especial interest to ascertain whether the quantity of marsh-gas extracted coincided in any way with the hydrogen obtained by their combustion. All the samples were therefore submitted to ultimate analysis, with the following results:—

	Percentage Composition.		
	Hydrogen.	Carbon.	Ash.
Celestial graphite	0·11	76·10	23·50
Borrodale „	0·11	94·76	4·85
Siberian „	0·17	79·07	20·00
Ceylon „	0·017	90·90	9·08
Unknown „	0·246	78·51	21·26

These analyses do not seem to point to any very definite conclusion as to the origin of the marsh-gas. The unknown graphite, which contains the largest percentage of marsh-gas, certainly comes out far the highest in hydrogen, and the hydrogen in the Ceylon graphite also bears a certain relation to the small quantity of marsh-gas it contains, but the first three samples are very similar to each other in the amount of hydrogen they contain.

In order to get some further insight into the origin of this marsh-gas in the celestial graphite, about 2 grams of the original nodule were very finely ground up and digested for several hours with strong nitric acid. After being thoroughly washed from every trace of nitric acid and dried at 110° C., it was again submitted to analysis, with the result that the amount of hydrogen remained exactly the same as before, proving that it existed in the form of some very stable compound in the graphite.

To clear up this matter still further, about 10 grams of the original nodule were digested with pure ether in the way described by Lawrence Smith for extracting the hydrocarbon-like bodies. It was allowed to stand for twenty-four hours with excess of ether, and then filtered, and washed with more ether. The graphite thus treated was dried at 110° C., and the gases extracted from it.

For the purpose of comparing one of the terrestrial graphites with the above in regard to its behaviour with ether, the specimen of unknown origin was selected, as yielding the largest quantity of marsh-gas. The residue, after digestion with ether, was dried, and the gases pumped out as before.

It will be seen that by this treatment with ether the volume of gas given off by the celestial graphite, and also the marsh-gas, have been reduced to rather more than one-half, while with regard to the unknown graphite, although the total volume of gas remains about the same (probably due to a rather higher temperature being employed), the marsh-gas has also been reduced to rather less than one-third the original amount, and the hydrogen has correspondingly increased.

	Occluded Gases in vols. of the Graphite.	CO_2.	CO.	H.	CH_4.	N.
Celestial graphite before treatment with ether ..	7·25	91·81	..	2·50	5·40	0·1
Celestial graphite after treatment with ether	3·50	81·50	10·63	1·41	2·12	0·74
Unknown graphite before treatment with ether ..	7·26	50·79	3·16	2·50	39·53	3·49
Unknown graphite after treatment with ether ..	7·15	64·86	5·67	14·37	12·96	2·00

These experiments prove that either the ether did not dissolve out all the actual carbonaceous compounds present, or that the marsh-gas was subsequently formed during the heating of the graphite.

As Dr. de la Rue had kindly placed at Professor Dewar's disposal a splendid specimen of the Orgueil meteorite, the opportunity was taken of comparing the gases occluded by this typical specimen of the carbonaceous class with those obtained from the stony meteorites and the graphites. This meteorite has been so thoroughly examined by Clöez and Pisani (Compt. Rend. vol. lix. (1864) pp. 37, 132) with regard to its chemical inorganic constituents, that nothing need be said as to its general composition. The investigation was therefore confined to the gases given off on heating which had not previously been determined.

During the heating of the meteorite a large quantity of water, on which floated numerous small pieces of sulphur, collected in the bulb tube immersed in the freezing mixture. This water was strongly acid, and indeed smelt strongly of sulphurous acid. On evaporating it to dryness with a drop of hydrochloric acid, abundance of ammoniacal salts were found in the residue. In the cool anterior part of the combustion-tube a considerable sublimate had collected, which proved to be principally sulphate of ammonium with traces of sulphides and sulphites, and a large quantity of free sulphur. A very large quantity of gas was given off, having the following composition:—

	Sp. gr.	Occluded Gases in vols. of the Meteorite.	Percentage Composition.				
			CO_2.	CO.	CH_4.	N.	SO_2.
Orgueil meteorite ..	2·567	57·87	12·77	1·96	1·50	0·56	83·00

Sulphurous acid is evidently the main constituent of the gases given off; but if this gas, which has been formed from the decomposition of the sulphate of iron, be eliminated, the meteorite yields 9·8 times its volume of gas, having very much the same composition as that from some of the stony meteorites, viz.:—

CO_2, 76·05; CO, 11·67; CH_4, 8·93; N, 3·33.

Clöez found the organic matter in this meteorite to be composed of carbon 63·45, hydrogen 5·98, oxygen 30·57, which is nearly in the proportions of a terrestrial humus substance. It is known that such substances break up by the action of heat into gases of the nature found above, at the same time, however, a quantity of the carbonic acid undoubtedly comes from the presence of the carbonates of magnesium and iron. The operation by which terrestrial carbon has been changed into graphite is by no means clear. As a rule the transition of one kind of carbon into another necessitates the action of a very high temperature. If, therefore, a really high temperature is in all cases necessary, it is difficult to explain how compounds of carbon came to resist decomposition, and should come to be found associated with all natural graphites.

It may be assumed that the graphite resulted from the action of water, gases and other agents, on the carbides of the metals, and that during the chemical interactions which took place, a portion of the carbon became transformed into organic compounds.

In either case it points to the conclusion that the method of formation of the meteoric and terrestrial graphites was similar, and it is perfectly possible they may after all have come from a common source.

It is proposed to continue this investigation, and in order to acquire further information, to examine the gases given off from meteorites at definite temperatures, and especially the gases from such as can be found coated with an impervious glaze, and to examine more particularly into the presence of water in such bodies, and the source of the nitrogen found in the same.

Since the above analyses of different graphites were made, a sample of the artificial graphite which results from the action of oxidising agents on the cyanogen compounds present in crude caustic soda has been examined. The following analysis shows that this artificial variety of graphite is characterised by giving a very large yield of marsh-gas.

CO_2	45·42
CO	39·88
CH_4	4·43
H	8·31
N	2·00

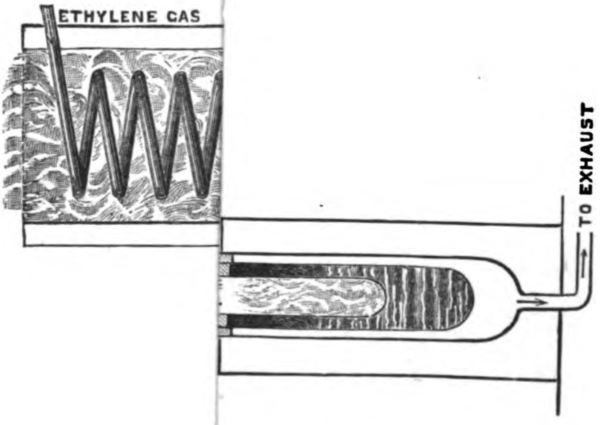
Occluded gases in volumes of the graphite = 53·13.

Meteorites, no doubt, have an exceedingly low temperature before they enter the earth's atmosphere, and the question had been raised as to what chemical reactions could take place under such conditions. It resulted from Professor Dewar's investigations that at a temperature of about − 130° C. liquid oxygen had no chemical action upon hydrogen, potassium, sodium, phosphorus, hydriodic acid, or sulphydric acid. It would appear, therefore, that as the absolute zero is approached even the strongest chemical affinities are inactive.

The lecturer exhibited at work the apparatus by which he had recently succeeded in solidifying oxygen. The apparatus is illustrated in the accompanying diagram,* where a copper tube is seen passing through a vessel kept constantly full of ether and solid carbonic acid; ethylene is sent through this tube, and is liquefied by the intense cold; it is then conveyed by the tube, through an indiarubber stopper, into the interior lower vessel; the outer one is filled with ether and solid carbonic acid. A continuous copper tube, about 45 feet long, conveying oxygen, passes first through the outer vessel, and then through that containing the liquid ethylene; the latter evaporates through the space between the two vessels, and thus intense cold is produced, whereby oxygen is liquefied in the tube to the extent occasionally of 22 cubic centimetres at one time. The temperature at which this is effected is about − 130° C., at a pressure of 75 atmospheres, but less pressure will suffice. When the oxygen is known to be liquid, by means of a gauge near the oxygen inlet, the valve A is opened, and the liquid oxygen rushes into a vacuum in the central glass tube below; some liquid ethylene at the bottom of the next tube outwards is also caused to evaporate into a vacuum at the same moment, and instantly some of the liquid oxygen in the central tube becomes solid, owing to the intense cold of the double evaporation.

---

* This illustration appeared in 'Industries' of July 16, 1886, and is kindly lent by the proprietors.


ETHYLENE GAS
TO EXHAUST

To face p. 550.

The outer glass vessel serves to keep moisture from settling on the sides of the ethylene tube. By means of the electric lantern and a lens, an image of this part of the apparatus was projected upon the screen, this being the first time that the experiment had been shown on a large scale in public.

Performing the experiment, the temperature reached was a little below 200° C., that is only 50° to 70° above the absolute zero of temperature, and in the experiment about 5 lbs. of liquid ethylene were employed.

With reference to the main subject, the lecturer said that meteorites came from regions of intense cold into our atmosphere; most of them weigh but a few ounces or pounds, but exceptional meteorites weigh several hundredweight. A spherical body 3 feet in diameter, moving at the rate of 18 miles a second at the height of 23 miles, where the barometric pressure is only two-tenths of an inch, produces locally a compression pressure 5600 times greater than that of the surrounding air. Descending vertically, it would pass through the whole atmosphere in 15 seconds. The velocity stated in these data is relatively low as compared with that of planetary bodies. Meteorites travel at the rate of about 36 miles in a second. The velocity of a shot from a 100-ton gun is about half a mile per second.

Meteorites reach the earth covered with a thin and very remarkable glaze, due to the fusion of their external surface during their brief passage through the atmosphere. A velocity of 145 feet per second in air gives an increase of 10° temperature, and the rate continues as the square of the velocity. The surface temperature of a body moving at the rate of 39 miles per second would reach 2,000,000°.

The lecturer placed a piece of iron against a rotating emery wheel, the friction of which caused showers of sparks to be thrown out. These were so hot that some of the little globules of iron composing them were fused into a plate of glass placed to catch them. Great similarities exist between the flight of these globules and the flight of meteorites, the heat and light in both cases being partly due to friction and partly to chemical action. That chemical action has an influence, he proved by applying oxygen gas to the sparks, thereby causing them to burn more brilliantly, and by applying carbonic acid to them, thus reducing their brilliancy. When a piece of meteorite was applied to the emery wheel in place of the piece of iron, the sparks were far less abundant, and of a dull red colour. The glaze of meteorites can be imitated to some extent by cooling a piece of meteorite to 200° C., and then dropping it for a moment into the electric furnace; the temperature explains the glazing of a meteorite, and that it has a motion of rotation must also be considered in estimating the amount of friction, and therefore of heat, to which it is subjected in its passage through the atmosphere of the earth. An enormous amount of its energy, however, is expended in heating the air, and aërial vibrations thus set up explain the noises made by the passage of meteorites.

What is the origin of the gases in meteorites? Their presence agrees with the discovery of Dr. Huggins, that comets give a hydrocarbon spectrum. The origin of terrestrial graphite is far from being agreed upon by geologists; in some places it is evidently transformed coal, in other cases they cannot say whether it comes from vegetable or primitive sources. Whatever the origin of the graphite in meteorites, it contains similar impurities to those in terrestrial graphite; the nodules in celestial graphite are similar to those of terrestrial graphite, and might as well have come from some body like the earth as from any other source. Another conclusion is that the marsh-gas is not occluded in meteorites, but is a product of distillation by heat, just as the gas might be distilled from shales. The graphite of meteorites has no power of occluding marsh-gas, therefore the inference is that the marsh-gas and hydrogen in them are the result of the decomposition of organic bodies. In the spectrum of one of the comets, Dr. Huggins once photographed a peculiar band in the ultra-violet, which band indicated the presence of cyanogen. One meteorite on the table contained chloride of ammonium, therefore it contained a compound of nitrogen, and such would account for the production of cyanogen.

[J. D.]

WEEKLY EVENING MEETING,

Friday, January 22, 1886.

SIR WILLIAM BOWMAN, Bart. LL.D. F.R.S. Vice-President,
in the Chair.

PROFESSOR TYNDALL, D.C.L. LL.D. F.R.S. *M.R.I.*

Thomas Young.

EARLY LIFE AND STUDIES.

FOUR great names are indissolubly associated with the establishment in which we are now assembled. Its founder, Benjamin Thompson, better known as Count Rumford; its Chemical Professor, Humphry Davy; its Professor of Natural Philosophy, Thomas Young; and, finally, the man whom so many of us have the privilege to remember, Michael Faraday. Of the character and achievements of the third of the great men here named, less seems to be publicly known than ought to be known. Even a portion of this audience may possibly have some addition made to its knowledge by reference to the labours of a man who served the Institution in the opening of the present century. I therefore thought that such a brief account of him as I could compress into an hour might not be without interest and instruction at the present time and place.

Thomas Young was born at Milverton, in Somersetshire, on the 13th of June, 1773. His parents were members of the Society of Friends. Nearly seven years of his childhood were spent with his maternal grandfather. He soon evinced a precocity which might have been expected to run to seed and die rapidly out. When two years old he was able to read with considerable fluency, and before he had attained the age of four years, he had read the Bible twice through. At the age of six he learnt by heart the whole of Goldsmith's 'Deserted Village.' His first formal teachers were not successful, and his aunt in those early days appears to have been more useful to him than anybody else. When not quite seven years of age, he was placed at what he calls a miserable boarding school at Stapleton near Bristol. But he soon became his own tutor, distancing in his studies those who were meant to teach him.

In March 1782, he was sent to the school of Mr. Thompson, at Compton, in Dorsetshire, of whose liberality and largeness of mind Young spoke afterwards with affectionate recognition. Here he worked at Greek and Latin, and read a great many books in both languages. He also studied mathematics and book-keeping. Of pregnant influence on his future life was the reading of Martin's 'Lectures on Natural Philosophy,' and Ryland's 'Introduction to the Newtonian

Philosophy.' He read with particular delight the optical portions of Martin's work. An usher of the school, named Jeffrey, taught him how to make telescopes, and to bind books. Here the early years of Young and Faraday inosculate, the one, however, pursuing bookbinding as an amusement, and the other as a profession. Young borrowed a quadrant from an intelligent saddler named Atkins, and with it determined the principal heights in his neighbourhood. He took to botany for a time, but was more and more drawn towards optics. He constructed a microscope. The disentangling of difficult problems was his delight. Seeing some fluxional symbols in Martin's work, he attacked the study of fluxions. Priestley on Air fascinated him. The Italian language was mastered by the aid of one of his schoolfellows named Fox.

After leaving Compton, he devoted himself to the study of Hebrew. Mr. Toulmin, of whom Young speaks with affection, lent him grammars of the Hebrew, Chaldee, Syriac, and Samaritan languages, all of which he studied with diligence and delight. Mr. Toulmin also lent him the Lord's Prayer in more than a hundred languages, the examination of which, Young declares, gave him extraordinary pleasure. Through one of those accidents which enter so largely into the tissue of human life, Young found himself at Youngsbury, near Ware in Hertfordshire. It was a strong testimony to his talent and character, that Mr. Barclay here accepted him as the preceptor of his grandson, Mr. Hudson Gurney, although Young was then little more than fourteen, and his pupil only a year and a half younger than himself. Thus began a life-long friendship between him and Hudson Gurney. Young spent five years at Youngsbury, which he deemed the most profitable years of his life. He spent the winter months in London, visiting booksellers' shops and hearing occasional lectures. He kept a journal in Hertfordshire, the first entry of which informs us that he had written out specimens of the Bible in thirteen different languages. It is recorded of Young that, when requested by an acquaintance, who presumed somewhat upon his youthful appearance, to exhibit a specimen of his handwriting, he very delicately rebuked the inquiry by writing a sentence in his best style in fourteen different languages.

Although the catalogue of Young's books might give the impression that he was a great reader, his reading was comparatively limited; but whatever he read, he completely mastered. Fichte compared the reading of reviews to the smoking of tobacco, affirming that the two occupations were equally pleasant, and equally profitable. Young, in this sense, was not a smoker. Whatever study he began, he never abandoned; and it was, says Dean Peacock in his 'Life of Young,' to his steadily keeping to the principle of doing nothing by halves, that he was wont in after life to attribute a great part of his success as a scholar and a man of science.

Young's mother was the niece of Dr. Brocklesby, and this eminent London physician appears to have taken the greatest interest in the development of his youthful relative. He, nevertheless, occasionally gives Young a rap over the knuckles for what he calls his "prudery." We all know the strenuous and honourable opposition that has been always offered to negro slavery by the Society of Friends. In carrying out their principles, they at one time totally abstained from sugar, lest by using it they should countenance the West Indian planters. Young here imitated the conduct of his sect, which Dr. Brocklesby stigmatised as "prudery." "My late excellent friend Mr. Day," says the Doctor, "the author of 'Sandford and Merton,' abhorred the base traffic in human lives as much as you can do; and even Mr. Granville Sharp, one of the earliest writers on the subject, has not done half as much service as Mr. Day in the above work. And yet Mr. Day devoured daily as much sugar as I do. Reformation," adds the Doctor, "must take its rise elsewhere, if ever there is a general mass of public virtue sufficient to resist such private interests."

Over and above his classical reading, from 1790 to 1792, Young read Simpson's Fluxions, the Principia and Optics of Newton, and many of the works of other famous authors, including Bacon, Linnæus, Boerhaave, Lavoisier, Higgins, and Black. He seems to have confined himself to works of the highest stamp. He mastered Corneille and Racine, read Shakespeare, Milton, Blackstone, and Burke. But he was, adds his biographer, "contented to rest in almost entire ignorance of the popular literature of the day."

I must, however, hasten over the early years and acquirements of this extraordinary personality. During his youth, he had none of the assistance which is usually within the reach of persons of position in England. All that I have here mentioned, and a vast deal more, he had acquired without having ever entered a public school, or touched a University. As a classic, he was, we are assured, both precise and profound. As a mathematician, he was many-sided, original, and powerful. Such an education, however, though well calculated to develop the strength of the individual, was not, in Peacock's opinion, the best calculated to put Young into sympathy with the mind of his age. "He was, throughout life, destitute of that intellectual fellow-feeling (if the phrase may be used), which is so necessary to form a successful teacher or lecturer, or a luminous and successful writer."

Young was intended for the medical profession, and his medical studies began in 1792. He came to London, and attended the lectures of Dr. Baily, Mr. Cruikshanks, and John Hunter. He made the acquaintance of Burke, Windham, Frederick North, Sir Joshua Reynolds, and Dr. Lawrence. By the advice of Burke he studied the philosophical works of Cicero. The bent of Young's moral character may be inferred from the quotations which he habitually entered in his commonplace book. Here is one of them:—"For my part," says Cicero, "I think the man who possessed that strength of mind, that constitutional tendency to temperance and virtue, which would lead him to avoid all enervating indulgences, and to complete the whole career of life in the midst of labours of the body and efforts of the mind; whom neither tranquillity nor relaxation, nor the flattering attentions of his equals in age and station, nor public games nor banquets would delight; who would regard nothing in life as desirable which was not united with dignity and virtue;—such a man I regard as being, in my judgment, furnished and adorned with some special gifts of the gods."

His medical studies were pursued with the same thoroughness which marked everything Young took in hand. He was an assiduous attendant at the best lectures. His delight in optics naturally drew him to investigate the anatomical structure of the eye. In regard to this structure most of you will remember that in front is the cornea, holding behind it the aqueous humour; then comes the iris, surrounding the aperture called the pupil, at the back of which we have the beautiful crystalline lens. Behind this, again, is the vitreous humour which constitutes the great mass of the eye. Thus, optically considered, the eye is a compound lens of great complexity and beauty. Behind the vitreous humour is spread the screen of the retina, woven of fine nerve-fibres. On this screen, when any object looked at is distinctly seen, a sharply defined image of the object is formed. Definition of the image is necessary to the distinctness of the vision. Were the optical arrangements of the eye rigid, distinct vision would be possible only at one definite distance. But the eye can see distinctly at different distances. It has what the Germans call an Accommodations Vermögen—a power of adjustment—which liberates it from the thrall of rigidity. By what mechanical arrangement is the eye enabled to adjust itself both for near and distant objects? Young replied, "By the alteration of the curvature of the crystalline lens." His memoir on this subject was considered so meritorious, that it was printed in the 'Transactions of the Royal Society'; and in the year following, at the age of twenty-one, he was elected a Fellow of the Society.

Young's memoir evoked sharp discussion, both as regards the priority and the truth of the discovery. It was claimed by John Hunter, while its accuracy was denied by Hunter's brother-in-law, Sir Everard Home, who, jointly with Mr. Ramsden, affirmed that the adjustment of the eye depended on the changed curvature of the cornea. A couched eye, that is to say, an eye from which the crystalline lens had been removed, they affirmed to be capable of adjustment. In the face of such authorities, Young, with the candour of a true man of science, abandoned the views he had enunciated. But it was only for a time. He soon resumed his inquiries, and proved to demonstration that couched eyes had no trace of the power ascribed to them. Before the time of Young, moreover, weighty authorities leaned to the view that the adjustment of the eye depended on the variation of the distance between the cornea and the retina. When near objects were viewed, it was thought that the axis was lengthened, the retina or screen being thereby thrown further back. In distant vision the reverse took place. But Young proved beyond a doubt that no such variation in the length of the axis of the eye occurs; and this has been verified in our own day by Helmholtz. The change in the curvature of the crystalline lens has been also verified by the most exact experiments. When we pass, for instance, from distant to near vision, the image of a candle flame reflected from the front surface of the lens becomes smaller, proving the lens to be then more sharply curved. When we pass from near to distant vision, the image becomes larger, proving the curvature of the lens to have diminished. The radius of curvature of the lens under these circumstances has been shown to vary from six to ten millimetres. The theory of Young, therefore, with regard to the adjustment of the eye, has been completely verified. But it is still a moot point as to what the mechanism is by which the change of curvature is produced. Young thought that it was effected by the muscularity of the lens itself. The muscles, however, would require nerves to excite them, and it would be hardly possible, in the transparent humours of the eye, for such nerves to escape detection. They, however, have never been detected.

While passing through Bath in 1794, Young, at the instance of Dr. Brocklesby, called upon the Duke of Richmond. The impression made by Young at this time may be gathered from a note addressed by the Duke to the Doctor, in these terms:—"But I must tell you how pleased we all are with Mr. Young. I really never saw a young man more pleasing and engaging. He seems to have already acquired much knowledge in most branches, and to be studious of obtaining more. It comes out without affectation on all subjects he talks upon. He is very cheerful and easy, without assuming anything; and even on the peculiarity of his dress and Quakerism, he talked so reasonably, that one cannot wish him to alter himself in any one particular. In short, I end, as I began, by assuring you that the Duchess and I are quite charmed with him." The Duke, then Master of the Ordnance, was a very competent man. He was well acquainted with the instruments used in the great Trigonometrical Survey under his control. He offered to Young the post of private secretary. Young's acceptance would have brought within his reach both honour and emolument. But, to his credit be it recorded, he refused the post, because its acceptance would have rendered necessary the abandonment of his costume as a member of the Society of Friends. Soon afterwards, he paid a visit to a celebrated cattle-breeder near Ashbourne, and describes with vivid interest what Mr. Bickwell had accomplished by the process of artificial selection. Facts like these it was which, presented afterwards to the pondering mind of Darwin, caused the great naturalist to pass from artificial to natural selection. Young visited Darwin's grandfather, and criticized his 'Zoonomia.' The inspection of Dr. Darwin's cameos, minerals, and plants, gave him great delight, the supreme pleasure being derived from the cameos. Dr. Darwin stated that he had borrowed much of the imagery of his poetry from the graceful expression and vigorous conception which these cameos breathe. His opinion of his visitor is pithily expressed in a letter of introduction to a friend in Edinburgh: "He unites the scholar with the philosopher, and the cultivation of modern arts with the simplicity of ancient manners."

Young went to Edinburgh to pursue his medical studies. His reputation had gone before him, and he was welcomed in the best society of the northern capital. Here he met Bostock, Bancroft, Turner, Gibbs, Gregory, Duncan, Black, and Munroe. He dwells specially upon the lectures of John Bell, whose demonstrations in anatomy appeared to him to be of first-rate excellence.

There is nothing that I have met in Dean Peacock's 'Life of Young,' to denote that he was fervently religious. The Ciceronian "virtue," rather than religious emotion, seemed to belong to his character. The hold which mere habit long exercised over him, and which his loyalty had caused him to maintain at a period of temptation, became more and more relaxed. He gradually gave up the formal practices of Quakerism, in regard to dress and other matters. He took lessons in dancing, and appeared to delight in that graceful art. I remember the late Mr. Babbage telling me that once, upon a London stage, by the untimely raising of a drop-scène, Young was revealed in the attitude of a dancer. He assiduously attended the theatre. So, it may be remarked, did the profoundly religious Faraday. On leaving Edinburgh he paid a farewell visit to his friend Cruikshanks, who took him aside, and after much preamble, "told me," says Young, "that he had heard that I had been at the play, and hoped that I should be able to contradict it. I told him that I had been several times, and that I thought it right to go. I know," he added, "you are determined to discourage my dancing and singing, and I am determined to pay no regard whatever to what you say."

After completing his studies at Edinburgh, Young went to the Highlands, and the houses in which he was received show the consideration in which he was held. He visited the chief seats of learning, and the principal libraries, as a matter of course; but he had also occasion to enjoy and admire "the good sense, frankness, cordiality of manners, personal beauty, and accomplishments" of the Scottish aristocracy. So greatly was he delighted with his visit to Gordon Castle, that before quitting it, he wrote thus: "I could almost have wished to break or dislocate a limb by chance, that I might be detained against my will. I do not recollect that I have ever passed my time more agreeably, or with a party whom I thought more congenial to my own disposition." He visited Staffa, but appears to have taken more pleasure in Pennant's plates and descriptions, than in Fingal's Cave, or the scenery of the island. From the Duchess of Gordon he carried a letter of introduction to the Duke of Argyll, and spent some time at Inverary. In riding out he was given his choice whether to proceed leisurely with the Duke, or to ride with the ladies and be galloped over. His reply was, that of all things, he liked to be galloped over, and made his choice accordingly. He compares the two daughters of the Duke to Venus and Minerva, both being goddesses. He visited the Cumberland lakes. But here it may be said, once for all, that Young was somewhat stunted in his taste for natural scenery. He was a man of the town, fond of social intercourse, and of intellectual collision. He could not understand the possibility of any man choosing to live in the country if the chance of living in London was open to him. At Liverpool he dined with Roscoe, proceeding afterwards to Coalbrookdale and its ironworks. As previously at Carron, he was greatly impressed by the glare of the furnaces. Mr. W. Reynolds, who appeared to interest himself in physical experiments on a large scale, told him that he had the intention of making a flute 150 feet long and 2½ feet in diameter, to be blown by a steam engine and played upon by barrels. From Young's letters, it is evident that he then saw the value and necessity of what we now call technical education.

In October 1795, he went to Germany to pursue his medical studies at the University of Göttingen. He gives an account of his diurnal occupations, embracing attendance at lectures on history, on materia medica, on acute diseases, and on natural history. He is careful to note that he had also lessons twice a week from Blessmen, the academical dancing-master, and the same number of lessons on the clavichord from Forkel. Young's pursuit of "personal accomplishments" is considered by his biographer to be somewhat excessive. At Göttingen he had, on Sundays, tea dances or supper dances. The mothers of handsome daughters appear to have been somewhat wary of the students, having reason "to fear a traitor in every young man." He made at Göttingen the acquaintance of many famous professors—of Heyne, Lichtenberg, Blumenbach, and others. He records a joke practised on the professor of geology which had serious consequences. The students were rather bored by the professor's compelling them to go with him to collect "petrifactions;" and the young rogues, says Young, "in revenge, spent a whole winter in counterfeiting specimens and buried them in a hill which the good man meant to explore, and imposed them upon him as the most wonderful *lusus naturæ.*" Peacock adds the remark that the unhappy victim of this "roguery" died of mortification when the imposition was made known to him.

Before taking his degree, it is customary for the student in German Universities to hand in a dissertation written by himself. This is circulated among the professors, and is followed by a public disputation. On the 16th of July, Young did battle in the Auditorium, the subject chosen for discussion being the human voice. He acquitted himself creditably, was complimented by those present, and received his degree as doctor of physic, surgery, and midwifery. In the thesis chosen for discussion, Young broke ground on those studies on sound which, for intrinsic merit, and suggesting as they did, his subsequent studies on light, will remain for ever famous in the history of science. During a pause in the lectures he visited the Hartz mountains, making himself acquainted with the scene of Göthe's Walpurgisnacht on the summit of the Brocken. Wedgwood and Leslie accompanied him on this tour. The curious fossils dug up by the young men in the Unicorn's Cave at Schwarzfeld, excited curiosity and wonder, but nothing more. Their significance at that time had not been revealed. Hearing Kant so much spoken of in Germany, Young naturally attacked the 'Critique of Pure Reason,' but his other studies prevented him from devoting much time to the critical philosophy. To the portion of it which he read he attached no high value. He admitted Kant's penetration, but dwelt upon his confusion of ideas. The language of the Critique he thought unpardonably obscure.

He visited Brunswick, where, clothed in the proper costume, he was presented at court. After the reception came a supper, about twenty ladies sitting on one side of a table, and twenty gentlemen on the other. He endeavoured to converse with his neighbour, but found him either sulky or stupid. The dowager duchess, whom he likened to a spectre, made her appearance, and began to converse pleasantly. When told that Young had studied at Göttingen, and that he was a doctor of medicine she asked him whether he could feel a pulse, and whether the English or the Germans had the best pulses. Young replied that he had felt but one pulse in Germany, the pulse of a young lady—and that it was a very good pulse. Göttingen was then the foremost school of horsemanship in Europe. Young was passionately fond of this exercise, and there were no feats of horsemanship, however daring or difficult, which he did not attempt or accomplish. His muscular power had been always remarkable, and he could clear a five-bar gate without touching it. He was better known among the students for his vaulting on a wooden horse than for writing Greek, regarding which they had neither knowledge nor respect. At a court masquerade he appeared in the character of harlequin, which gave him an excellent opportunity of exhibiting his personal activity. Notwithstanding all this, he did not quite like his life in Göttingen. The professors of the University were worked too hard to leave much time for the receptions and social gatherings in which Young delighted. So he quitted Göttingen on the 28th of August, "with as little regret as a man can leave any place where he has resided nine months."

From Göttingen he walked to Cassel, and thence by Gotha, Erfurt, Weimar, and Jena, to Leipzig. He saw everything which to him was worth seeing, and as he carried letters of introduction from the most eminent men of the age, he was welcomed everywhere. Most of the professors were absent on their holiday, but at Weimar he conversed with Herder, who, though well versed in the English poets, cared nothing, it is said, about rhyme. At Jena he found Bütmer, who, at the age of eighty-three, was about to begin the publication of a general dictionary of all existing languages. He visited Dresden, the Saxon Switzerland, and the mines of Freiberg. Here he made the acquaintance of the celebrated Werner. From Freiberg he went to Berlin, where he dined twice with the English Ambassador, Lord Elgin, and once with Dr. Brown, a Welsh physician, in great favour with the King. Over the monotonous sandy flat that lies between the two cities, he journeyed from Berlin to Hamburg. Detained here for a time by adverse winds, he was treated with great hospitality.

One word in conclusion regarding the German schools of learning. Germany is now united and strong, her sons are learned, and her prowess is proved. But the units from which her blended vigour has sprung ought not to be forgotten. These were the little principalities and powers of which she was formerly composed. Each of them asserted its individuality and independence by the establishment of a local University, and all over Germany, in consequence, such institutions are sown broadcast. In these nurseries of mind and body, not only Bismarck and von Moltke, but numbers of the rank and file of the German army found nutriment and discipline; so that though, as long as her principalities remained separate, Germany as a whole was weak, the individual action of those small states so educated German men as to make them what we now find them to be.

Two epochs of Young's career as a medical student have been now referred to—his residence in Edinburgh, and his residence at Göttingen. Immediately after his return to England he became a fellow-commoner of Emanuel College, Cambridge. When the master of the college introduced him to those who were to be his tutors, he jocularly said, "I have brought you a pupil qualified to read lectures to his tutors." On one occasion, in the Combination Room, Dr. Parr made some dogmatic observation on a point of scholarship. "Bentley, sir," said Young promptly and firmly, "was of a different opinion." "A smart young man that," said Parr when Young quitted the room. His lack of humour, and want of knowledge of popular literature, sometimes made him a butt at the dinner-table, but he bore the banter with perfect good humour. The materials for Young's life at Cambridge are very scanty; but there is one brisk and energetic letter, published by Dean Peacock, written by a man who was by no means partial to Young. "Young," he said, "was beforehand with the world in perceiving the defects of English mathematicians. He looked down upon the science, and would not cultivate the acquaintance of any of our philosophers. He seemed never to have heard the names of the poets and literary characters of the last century, and hardly ever spoke of English literature." According to Peacock's correspondent, there was about Young no pretence or assumption of superiority. "He spoke upon the most difficult subjects as if he took it for granted that all understood the matter as well as himself. But he never spoke in praise of any of the writers of the day, and could not be persuaded to discuss their merits. He would speak of knowledge in itself—of what was known or what might be known, but never of himself or of any one else, as having discovered anything, or as likely to do so. His language was correct, his utterance rapid, but his words were not those in familiar use, and he was therefore worse calculated than any man I ever knew for the communication of knowledge." This writer heard Young lecture at the Royal Institution, but thought that nothing could show less judgment than the method he adopted. "It was difficult to say how he employed himself at Cambridge. He read little; * there were no books piled on his floor, no papers scattered on his table. His room had all the appearance of belonging to an idle man. He seldom gave an opinion, and never volunteered one; never laid down the law, like other learned doctors, or uttered sayings to be remembered. He did not think abstractedly. A philosophical fact, a difficult calculation, an ingenious instrument, or a new invention, would engage his attention; but he never spoke of morals, or metaphysics, or religion. Of the last, I never heard him say a word. Nothing in favour of any sect or in opposition to any doctrine."

The impression made upon Young by Cambridge was, from first to last, entirely favourable. In those days, six years' study were indispensable before the degree of Bachelor of Medicine could be taken. Young graduated in 1803, when he was thirty years of age, and five years more had to elapse before he could take the degree of M.D. Meanwhile he had begun the practice of medicine. Dr. Brocklesby died, in 1797, on the night of a day when he had entertained his nephew and some other friends at dinner. During dinner he seemed perfectly well, but he expired a few minutes after he went to bed. He left Young his house and furniture in Norfolk Street, Park Lane, his library, his prints, a collection of pictures chiefly selected by Sir Joshua Reynolds, and about 10,000*l.* in money.

THE WAVE THEORY.

On the 16th of January, 1800, Young communicated to the Royal Society his memoir entitled "Outlines and Experiments respecting Sound and Light." In this paper he treated of the interference of sound, and his researches on this subject led him on to the discovery of the interference of light—"Which has proved," says Sir John Herschel, "the key to all the more abstruse and puzzling properties of light, and which would alone have sufficed to place its author in the highest rank of scientific immortality, even were his other almost innumerable claims to such a distinction disregarded." Newton considered the sensation of light to be aroused by the darting into the eye, and the impinging against the retina, of particles inconceivably minute. Huyghens, on the contrary, supposed the sensation of light to be aroused by the impact of minute waves against the retina. Young favoured the theory of undulation, and by his researches on sound he was specially equipped for its thorough examination. Before he formally attacked the subject, he gave, in a paper dealing with other matters, his reasons for espousing the wave theory. The velocity of light, for instance, in the same medium is constant. All refractions are attended with partial reflection. The dispersion of light is no more incompatible with this than with any other theory. Reflection and refraction are equally explicable on both suppositions. Huyghens, indeed, had proved this, and much more. Inflection may be better explained by the wave theory than by its rival. The colours of thin plates, which are perfectly unintelligible on the common hypothesis, admit of complete explanation by the wave theory. In dealing with the colours of thin films, of which the soap-bubble offers a familiar example, Young first proved his mastery over the undulatory theory. In the pursuit of this great task, he was able to convert Newton's Theory of Fits into the Theory of Waves, and to determine the lengths of the undulations corresponding to the different colours of the spectrum.

---

* Critics and commentators must be great readers; but *creators* in science and philosophy do not always belong to this category.

We now approach a phase of Young's career which more specially concerns us. The Royal Institution, as already stated, was founded by Count Rumford, supported by many of the foremost men in England. The King was its patron, the Earl of Winchilsea its first president, while Lord Morton, Lord Egremont, and Sir Joseph Banks were its vice-presidents. On the 13th of January, 1800, the Royal Seal was attached to the charter of the Royal Institution. Dr. Thomas Garnet was appointed Professor of Natural Philosophy and Chemistry. During his previous residence in Bavaria, Rumford had ruled with beneficent but despotic sway, and the habit of mind thus engendered, may have made itself felt in his behaviour to Dr. Garnet. At all events, they did not get on well together. On the 16th February, 1801, Davy was appointed Assistant Lecturer in Chemistry, Director of the Chemical Laboratory, and Assistant Editor of the Journals of the Institution. The post of Professor of Natural Philosophy was offered to Young, and he accepted it. The salary was to be 300*l.* a year. On the 3rd of August, 1801, the following resolution was passed:—"Resolved, that the Managers approve of the measures taken by Count Rumford, and that the appointment of Dr. Young be confirmed." Young, it is said, was not successful as a lecturer in the Institution, and this Dr. Peacock ascribes to his early education, which gave him no opportunity of entering into the intellectual habits of other men. More probably, the defect was due to a mental constitution, not plastic, like that of Davy or Faraday, in regard to exposition. Young now fairly fronted the undulatory theory of light. Before you is some of the apparatus he employed. I hold in my hand an ancient tract upon this subject by the illustrious Huyghens. It was picked up on a bookstall, and presented to me some years ago by my friend Professor Dewar. In this tract, Huyghens deals with refraction and reflection, giving a complete explanation of both; and here, also, he enunciated a principle which now bears his name, and which forms one of the foundation stones of the undulatory theory.

The most formidable obstacle to Young's advance, and one which he never entirely surmounted, was an objection raised by Newton to the assumption of a fluid medium as the vehicle of light. Looking at the waves of water impinging on an isolated rock, Newton observed that the rock did not intercept the wave motion. The waves, on the contrary, bent round the rock, and set in motion the water at the back of it. Basing himself on this and similar observations, he says, "Are not all hypotheses erroneous in which light is supposed to consist of a pression or motion propagated through a fluid medium? If it consisted in pression or motion, it would bend into the shadow." He instances the case of the sound of a bell being heard behind a hill which conceals the bell; of the turning of corners by sound; and then, with conclusive force he points to the case of a planet coming between a fixed star and the eye, when the star is completely blotted out by the interposition of the opaque body. This, Newton urged, could not possibly occur if light were propagated by waves through a fluid medium, for such waves would infallibly stir the fluid behind the planet, and thus obliterate the shadow.

Young was firmly persuaded of the truth of the undulatory theory. The number of riddles that, by means of it he had solved, the number of secrets he had unlocked, the number of difficulties he had crushed, rendered him steadfast in his belief; still, he never fairly got over this objection of Newton's; and it was first set aside by one of the most illustrious men that ever adorned the history of science. A young French officer of engineers, Augustin Fresnel, first really grappled with the difficulty and overthrew it. The principle of Huyghens, to which I have already referred, is, that every particle, in every wave, acts as if it alone were a centre of wave motion. When you throw a stone into the Serpentine, circular waves or ripples are formed which follow each other in succession, retreating further and further from the point of disturbance.* Fix your attention on one of these circular waves. The *form* of the wave moves forward, but the motion of its individual particles, at any moment, is simply a vibration up and down. Now each oscillating particle of every moving wave, if left to itself, would produce a series of waves, not so high, but in other respects exactly similar to those produced by the stone. The coalescence of all these small waves produces another wave of exactly the same kind as that which started them. The principle that every particle of a wave acts independently of all other particles, while the waves produced by all the particles afterwards combine, is, as I have said, the great principle of Huyghens. Taken in conjunction with the interference of light, first established by Thomas Young, which proved that when waves coalesce or combine, they may either support each other, or neutralise each other, the neutralisation being either total or partial, according as the opposition of the combining waves is complete or partial; taking, I say, the principle of interference in conjunction with that of Huyghens, Fresnel proved that although light does diverge behind an opaque body, as Newton supposed that it would diverge, these divergent waves completely efface each other, producing the shadow due to the tranquillity of the medium which propagates the light.

---

* "I prove it thus, take heed now
By experience, for if that thou
Threw in water now a stone
Well wost thou it will make anone,
A little roundell as a cercle,
Peraventure as broad as a couercle,
And right anone thou shalt see wele,
That whele cercle wil cause another whele,
And that a third and so forth brother,
Every cercle causing other."
Chaucer's 'House of Fame.'

By reference to the waves of water, Young illustrates, in the most lucid manner, the interference of the waves of light. He pictures two series of waves generated at two points near each other in a lake, and reaching a channel issuing from the lake. If the waves arrive at the same moment, neither series will destroy the other. If the elevations of both series coincide, they will, by their joint action, produce in the channel a series with higher elevations. But if the ridges of one series correspond to the depressions of the other, the ridges will exactly fill the depressions, smooth water in the channel being the result. "At least," says Young, "I can discover no alternative, either from theory or from experiment. Now," he continues —gathering confidence as he reasons, "I maintain that similar effects take place whenever two portions of light are thus mixed, and this I call the general law of the interference of light."

The physical meaning of all the terms applied to light was soon fixed. *Intensity* depended upon the *amplitudes* of the waves. *Colour* depended on the *lengths* of the waves. Two series of waves coalesced and helped each other when one was any number of complete undulations or, in other words, any *even* number of half-undulations, behind the other. Two series of waves extinguished each other when the one series was any *odd* number of semi-undulations behind the other. But inasmuch as white light is made up of innumerable waves of different lengths, such waves cannot all interfere at the same time. Some interfere totally, and destroy each other; some partially; while some add themselves together and enhance the effect. Thus, by interference, a portion only of the white light is withdrawn, and the remaining portion is, as a general rule, coloured. Indeed the most glowing and brilliant effects of coloration are thus produced. Young applied the theory successfully to explain the colours of striated surfaces which, in the hands of Mr. Rutherfurd and others, have been made to produce such splendid effects. The iridescences on the polished surfaces of mother-of-pearl are due to the striæ produced by the edges of the shell-layers, which are of infinitesimal thickness; the fine lines drawn by Coventry, Wollaston, and Barton upon glass also showed these colours. Barton afterwards succeeded in transferring the lines to steel and brass. Most of you are acquainted with the iridescence of Barton's buttons. A descendant of Mr. Barton has, I believe, succeeded in reproducing the instrument wherewith his grandfather produced his brilliant effects.

But the greatest triumph of Young in this field was the explanation of the beautiful phenomenon known as Newton's rings. The colours of thin plates were profusely illustrated by the experiments of Hooke and Boyle, but Newton longed for more than illustrations. He desired quantitative measurement. The colour of the film was known to depend upon its thickness. Can this thickness be measured? Here the unparalled penetration of Newton came into play. He took a lens consisting of a slice of a sphere of a diameter so large that the curved surface of the lens approximated to a plane surface. Upon this slightly convex surface he placed a plate of glass whose surface was accurately plane. Squeezing them together, and allowing light to fall upon them, he observed those beautiful iris-circles with which his name will be for ever identified. The iris-colours were obtained when he employed white light. When monochromatic light was used he had simply successive circles of light and darkness. Here then, from the central point where the two glasses touched each other, Newton obtained a film of air which gradually increased in thickness as he retreated from the point of contact. Whence this wonderful recurrence of light and darkness? The very constitution of light itself must be involved in the answer. His desire was now to ascertain the thickness of the film of air corresponding to the respective rings. Knowing the curvature of his lens, this was a matter of easy calculation. He measured the diameter of the fifth ring of the series. This might be accurately done with a pair of fine compasses, for the diameter was over the fifth of an inch in length. But it was the interval between the glasses corresponding to this distance that Newton required to know, and this he found by calculation to be 1–37,000th of an inch. This, be it remembered, is the distance corresponding to the fifth ring. The interval corresponding to the first ring would be only a fifth of this, or, in other words, about 1–180,000th of an inch. Such are the magnitudes with which we have to deal before the question "What is Light?" can be scientifically answered.

Newton's explanation of the rings, which he was the first to discover, though artificial in the highest degree, is marked by his profound sagacity. He was hampered by the notion of the "corporeity" of light. He could not get over the objection raised by himself as to the existence of shadows in a fluid medium. He held therefore that light was due to the darting forth of minute particles in straight lines; and he threw out the idea that colour might be due to the difference of bigness in the particles. He endowed these particles with what he called *fits* of easy transmission and reflection. The dark rings, in his immortal experiment, were produced where the light-particles were in their transmissive "fit." They went through both surfaces of the film of air, and were not thrown back to the eye. The bright rings occurred where the light-particles were in their reflective fit, and where, on reaching the second surface of the film, they were thrown back to the eye. The cardinal point here is, that Newton regarded the recurrence of light and darkness as due to an action confined to the *second surface* of the film. And here it was that Young came into irreconcilable collision with him, proving to demonstration that the dark rings occurred where the portions of light reflected from *both sides* of the film extinguished each other by interference, while the bright rings occurred where the light reflected from the two surfaces coalesced to enhance the intensity.

Young next applied the wave theory to account for the diffraction or inflection of light, that is to say, the effects produced by its bending round the edges of bodies. When a cone of rays issuing from a very minute point impinges on an opaque body, so as to embrace it wholly, the shadow of the body, if received upon a screen, exhibits fringes of colour. They follow so closely the contour of the opaque body, that Sir John Herschel compared them to the lines along the sea-coast in a map. If a very thin slip of card, or a hair, be placed within such a cone, it is noticed that besides the fringes outside the shadow, bands of colour occur within it; the central, or brightest band, being always white when white light is employed. It is a singular and somewhat startling fact, that by the interposition of an opaque body, say a small circle of tinfoil, the point on which we should expect the centre of the shadow to fall, is, by the joint action of diffraction and interference, illuminated in precisely the same degree as it is when the opaque circle is withdrawn.* In reference to the interior fringes, Young made the observation, which is of primary importance, that, if you intercept the light passing by one of the edges of the strip of card or of the hair, the fringes disappear. It requires the inflection of the waves round both edges of the object, and their consequent interference, to produce the fringes.

Young's attempt to explain the phenomena of diffraction was a distinct advance on the extremely artificial hypothesis of Newton. Still his attempt was not so successful as his explanation of the colours of striated surfaces and of thin, thick, and mixed plates. Here the young officer of engineers to whom I have already referred,

---

* A similar diffraction has been proved by Lord Rayleigh to occur in the case of sound.

Fresnel, entered the field. He presented in 1815, to the French Institute, a memoir on Diffraction, which marks an epoch in the history of the wave theory. It is usual, when such a paper is presented, to refer it to a "Commission" who consider it and report upon its merits. The *commissionaires* in this instance were Arago and Prony.

Arago had read the memoirs of Young in the 'Philosophical Transactions,' but had not understood their full significance. The study of Fresnel's memoir caused the full truth to flash upon him that his young countryman had been anticipated thirteen years previously by Dr. Young. Fresnel had re-discovered the principle of interference independently, and had applied it, with profound insight and unrivalled experimental skill, to the phenomena of diffraction. It was no light thing to Fresnel to find himself, as regards the principle of interference, suddenly shorn of his glory. He, however, bore the shock with resignation. He might have readily made claims which would have found favour with his countrymen and with the world at large. But he did nothing of the kind. The history of science, indeed, furnishes no brighter example of honourable fairness than that exhibited throughout his too short life by the illustrious young Frenchman. Once assured that he had been anticipated—whatever might have been the extent of his own labours, however independently he might have arrived at his results, he unreservedly withdrew all claim to the discovery. There is, I repeat, no fairer example of scientific honour than that manifested by Augustin Fresnel.

Fresnel was a powerful mathematician, and well versed in the best mathematical methods of his day. With enormous labour he calculated the positions where the phenomena of interference must display themselves in a definite way. He was, moreover, a most refined experimentalist, and having made his calculations, he devised instrumental means of the most exquisite delicacy, with the view of verifying his results. In this way he swept the field of diffraction practically clear of difficulty, solving its problems where even Young had failed.

Truly, these were minds possessing gifts not purchasable with money! and round about the central labours of both of them, minor achievements of genius are to be found, which would be a fortune to less opulent men. I hardly know a finer example of Young's penetration than his account of the spurious or supernumerary bows, observed within the true primary rainbow. These interior bows are produced by interference. It is not difficult, by artificial means, to form these bows in great number and beauty. This is a subject on which, as you are aware, I worked a couple of years ago myself. And often, when looking at these bows, the words of Young seemed to me like the words of prophecy. The bows were the physical transcript of what Young stated must occur; a transcript, moreover, which, when compared with his words, was far more complete and impressive than any ever exhibited by the rainbow in nature. Many of you are acquainted with the beautiful rings of colour observed when a point of light is looked at through the seeds of lycopodium shaken over a piece of glass, or shaken in the air so as to form a cloud whose particles are all of the same size. The iridescence of clouds that I have once or twice seen in great splendour in the Isle of Wight, but more frequently in the Alps, is due to this equality in the size of the cloud-particles. Now the smaller the particles, the wider are the coloured rings, and Young devised an instrument called the *Eriometer*, which enabled him, from the measurement of the rings, to infer the size of the particles. Again, Ritter had discovered the ultra-violet rays of the spectrum, while Wollaston had noticed the darkening effect produced by these rays when permitted to fall on paper or leather which had been dipped in a solution of muriate of silver. Employing these invisible rays to produce invisible Newton's rings, Young projected an image of the rings upon the chemically prepared paper. He thus obtained a distinct photographic image of the rings. This was one of the earliest experiments wherein a true photographic picture was successfully obtained. Young had little notion at the time of the vast expansions which the art of photography was subsequently to undergo.

But Young was not permitted to pursue his great researches in peace. The 'Edinburgh Review' had at that time among its chief contributors a young man of vast energy of brain and vast power of sarcasm, without the commensurate sense of responsibility which might have checked and guided his powers. His intellect was not for a moment to be measured with that of Young; but as a writer appealing to a large class of the public, he was, at that time, an athlete without a rival. He afterwards became Lord Chancellor of England. Young, it may be admitted, had given him some annoyance, but his retaliation, if such it were, was out of all proportion to Young's offence. Besides, whatever his personal feelings were, it was not Young that he assailed so much as those sublime natural truths of which Young at the time was the foremost exponent. Through the undulatory theory he attacked Young without scruple or remorse. He sneered at his position in the Royal Institution, and tried hard to have his papers excluded from the 'Philosophical Transactions.' "Has the Royal Society," he says, "degraded its publications into bulletins of new and fashionable theories for the ladies of the Royal Institution? Let the Professor continue to amuse his audience with an endless variety of such harmless trifles, but in the name of science let them not find admittance into that venerable repository which contains the works of Newton and Boyle and Cavendish and Maskelyne and Herschel." The profound, complicated and novel researches on which Young was then engaged, rendered an occasional change of view necessary. How does the reviewer interpret this praiseworthy loyalty to truth? "It is difficult," he says, "to deal with an author filled with a medium of so fickle and vibratory a nature. Were we to take the trouble of refuting him he might tell us, '*my opinion is changed, and I have abandoned that hypothesis. But here is another for you.*' We demand if the world of science which Newton once illuminated is to be as changeable in its modes as the world of fashion, which is directed by the nod of a silly woman or a pampered fop? . . . . . We have a right to demand that the hypothesis shall be so consistent with itself as not to require perpetual mending and patching; that the child we stoop to play with shall be tolerably healthy, and not of the puny and sickly nature of Dr. Young's productions, which have scarcely *stamina* to subsist until the fruitful parent has furnished us with a new litter, to make way for which he knocks on the head, or more barbarously exposes, the first." He taunts Young with claiming the inheritance of Newton's queries, "vainly imagining that he fulfils this destination by ringing changes on these hypotheses, arguing from them, as if they were experiments or demonstrations, twisting them into a partial coincidence with the clumsy imaginations of his own brain, and pompously parading what Newton left as hints, in a series of propositions, with all the affectation of system."

To Brougham's coarse invective Young replied in a masterly and exhaustive letter. A single copy, and one only, was sold by its publisher. There were at that time in the ranks of science no minds competent to understand the controversy. The poison worked without an antidote, and, for thirteen years, Young and his researches on light had no place in public thought. His discoveries remained absolutely unnoticed until their re-discovery by Fresnel lifted the pall which for so many years had been thrown over this splendid genius.

Young lectured for two years at the Royal Institution, and he afterwards threw the lectures into a permanent form in a quarto volume of 750 pages with 40 plates, and nearly 600 figures and maps. He also produced at the same time a second volume of the same magnitude, embracing his optical and other memoirs, and a most elaborate classed catalogue of works and papers, accompanied by notes, extracts, and calculations. For this colossal work Young was to receive 1000*l.* His publisher, however, became bankrupt, and he never touched the money. His lectures constitute a monument of Young's power almost equal to that of his original memoirs. They are replete with profound reflections and suggestions. In his eighth lecture, on "Collision," the term *energy*, now in such constant use, is first introduced and defined. By it he was able to avoid, and enable us to avoid, the confusion which had crept into scientific literature by the incautious employment of the word *force*. The theory now known as the Young-Helmholtz theory, which refers all the sensations of colour to three primary sensations—red, green, and violet—was clearly enunciated by Young in his thirty-seventh lecture, on "Physical Optics." His views of the nature of heat were original and correct. He regarded the generation of heat by friction as an unanswerable confutation of the whole doctrine of material caloric. He gives appropriate illustrations of the manner in which he supposed the molecules of bodies to be shaken asunder by heat. "All these analogies," he says, "are certainly favourable to the opinion of the vibratory nature of heat, which has been sufficiently sanctioned by the authority of the greatest philosophers of past times and by the most sober reasoners of the present." In anticipation of Dr. Wells, Young had observed and recorded the fact, that a cloud passing over a clear sky sometimes causes the almost instantaneous rise of a thermometer placed upon the ground. The cloud he assumed acted as a vesture which threw back the heat of the earth. Radiant heat and light are here placed in the same catagory. William Herschel had already shown their kinship, by proving that the most powerful rays of the sun were entirely non-luminous. Subsequent to this, the polarization of heat, by Principal James Forbes, rendered yeoman's service in the propagation of the true faith.

The passage in which Young introduces and defines the term *energy* is so remarkable, that I venture to reproduce it here.

"The term energy may be applied, with great propriety, to the product of the mass or weight of a body into the square of the number expressing its velocity. Thus, if a weight of one ounce moves with a velocity of a foot in a second, we may call its energy 1; if a second body of two ounces have a velocity of three feet in a second, its energy will be twice the square of three, or 18. This product has been denominated the living or ascending force, since the height of the body's vertical ascent is in proportion to it; and some have considered it as the true measure of the quantity of motion; but although this opinion has been very universally rejected, yet the force thus estimated well deserves a distinct denomination. After the considerations and demonstrations which have been premised on the subject of forces, there can be no reasonable doubt with respect to the true measure of motion; nor can there be much hesitation in allowing at once that since the same force, continued for a double time, is known to produce a double velocity, a double force must also produce a double velocity in the same time. Notwithstanding the simplicity of this view of the subject, Leibnitz, Smeaton, and many others, have chosen to estimate the force of a moving body, by the product of its mass into the square of its velocity; and though we cannot admit that this estimation of force is just, yet it may be allowed that many of the sensible effects of motion, and even the advantage of any mechanical power, however it may be employed, are usually proportional to this product, or to the weight of the moving body, multiplied by the height from which it must have fallen, in order to acquire the given velocity. Thus, a bullet moving with a double velocity, will penetrate to a quadruple depth in clay or tallow; a ball of equal size, but of one-fourth of the weight, moving with a double velocity, will penetrate to an equal depth; and, with a smaller quantity of motion, will make an equal excavation in a shorter time.

This appears at first sight somewhat paradoxical; but, on the other hand, we are to consider the resistance of the clay or tallow as a uniformly retarding force, and it will be obvious, that the motion, which it can destroy in a short time, must be less than that which requires a longer time for its destruction. Thus also when the resistance, opposed by any body to a force tending to break it, is to be overcome, the space through which it may be bent before it breaks being given, as well as the force exerted at every point of that space, the power of any body to break it is proportional to the energy of its motion, or to its weight multiplied by the square of its velocity."

Young's essay on the Cohesion of Fluids, is to be ranked amongst the most important and difficult of his labours. It embraced his views and treatment of the subject of capillary attraction. But as this topic is to be treated here next week by a spirit kindred to that of Young himself,* I may be excused for saying nothing more about it. The essay drew Young into a controversy with the illustrious La Place, in which the Englishman exhibited that scimitar-like sharpness of pen which more than once had drawn him into controversy.

Young resigned his post at the Royal Institution because of the conviction that his devotion to work alien to his profession would be sure to injure his prospects as a physician. In the summer of 1802 he visited Paris, and at one of the meetings of the Academy was introduced to the First Consul. In March 1803, he became M.B. of Cambridge—six years after entering the University—while five years more had to elapse before he was able to take the degree of M.D. In June 1804, he married Miss Eliza Maxwell, the daughter of J. P. Maxwell, Esq., of Trippendence, near Farnborough, in Kent.

As regards medical practice, Young was probably too cool and cautious in the examination of his data, and trusted too little to the lancet and the calomel invoked in the vigorous practice of his time, to be a popular physician. After a somewhat strenuous contest he was appointed Physician to St. George's Hospital, and the appointment was a strong proof of the esteem in which he was held. His lectures, however, were not so well attended as those of his colleagues, for he lacked the warmth and pliancy which usually commend a lecturer to young men. Young's medical works, embodying the results of great labour and research, were received with high consideration and esteem.

By the force of his sarcasm and the glamour of his rhetoric, Brougham had succeeded in inflicting a serious, if not irreparable wound, on the science of his country. After Young's crushing reply, which produced no effect whatever upon the public, the author of that reply was practically forgotten as a factor in the advancement of physical optics. But Science has always before her the stimulus of natural problems demanding solution, and after a temporary lull the desire to know more of the nature of light grew in force. New stars arose in France, while the strenuous industry and experimental discoveries of Brewster did much to hold us in equipoise with the Continent. In Paris, La Place, Malus, Biot, and Arago were all actively engaged. The three first proceeded strictly on the Newtonian lines, and by the memoir of La Place, on Double Refraction, all antagonism to the theory of emission was considered to be for ever overthrown. In the 'Quarterly Review,' Young criticised this memoir with sagacity and power, and his criticism remains valid to the present time. In accordance with the principles of the wave theory, Huyghens had given a solution of the problem of double refraction in Iceland spar. The solution was opposed to that of La Place. Dr. Wollaston, a man of the highest scientific culture and the most delicate experimental skill, subjected the theory of Huyghens to the severest metrical tests, and his results proved entirely favourable to that theory. Wollaston, however, lacked the boldness which would have made him a commander in those days of scientific strife. He saw opposed to him the names of Newton and La Place, and in the face of such authority he shrank from closing with the conclusions to which his own experiments so distinctly pointed.

---

* Sir William Thomson.

We now come to a critical point in the fortunes of the wave theory. I need not again refer to the difference between the motion of a wave and the motions of the particles which constitute a wave. A wave of sound, for instance, passing through the air of this room would have a velocity of about 1100 feet a second, while the particles which constitute the wave, and propagate it at any moment, may only move through inconceivably small spaces to and fro. Now, in the case of sound, this to-and-fro motion occurs *in the direction* in which the sound is propagated, and a little reflection will make it clear that no matter how a ray of sound, if we may use the term, is received upon a reflecting surface, it would be echoed equally all round as long as the angle inclosed between the reflecting surface and the ray remains unchanged. In other words, the sound ray has no *sides* and no preferences, as regards reflection. Now, Malus discovered that in certain conditions a beam of light shows such preferences. When caused to impinge upon a plane glass mirror, placed in a certain position, it may be wholly reflected; whereas when the mirror is placed in the rectangular position it may not be reflected at all.

Up to the hour when this discovery was made by Malus, light had been supposed to be propagated through ether exactly as sound is propagated through air. In other words, the direction in which the particles of ether were supposed to vibrate to and fro coincided with that of the ray of light. Those who had previously held the undulatory theory were utterly staggered by this new revelation, and their perplexity was shared by Young. He was for a time unable to conceive of a medium capable of propagating the impulses of light in a way different from the propagation of the impulses of sound. To ascribe qualities to the light-medium which would enable it to differ in its mechanical action from the sound-medium was an idea too bold—I might indeed say too repulsive—to the scientific mind to be seriously entertained. Yet, deeply pondering the question, Young was at length forced to the conclusion that the vibrations concerned in the propagation of light were executed *at right angles* to the direction of the ray. By this assumption of transverse vibrations, which removed all difficulty, Young also removed the ether from the class of aeriform bodies, and endowed it with the properties of a semi-solid.

Fresnel's memoir on Diffraction, upon which, as already stated, Arago had reported, initiated a lasting friendship between the two illustrious Frenchmen. They subsequently worked together. Fresnel, the more adventurous and powerful spirit of the two, came independently to the same conclusion that Young had previously enunciated. But so daring did the idea of transverse vibrations appear to Arago—so inconsistent with every mechanical quality which he could venture to assign to the ether—that he refused to allow his name to appear in conjunction with that of Fresnel on the title-page of the memoir in which this heretical doctrine was broached. Still, the heresy has held its ground, and the theory of transverse vibrations as applied to the ether is now universally entertained.

Fresnel died in the fortieth year of his age.

Allow me to wind up this section of our labours by reference to a German estimate of Young's genius. "His mind," says Helmholtz, "was one of the most profound that the world has ever produced; but he had the misfortune of being too much in advance of his age. He excited the wonder of his contemporaries who, however, were unable to rise to the heights at which his daring intellect was accustomed to soar. His most important ideas lay, therefore, buried and forgotten in the folios of the Royal Society, until a new generation gradually and painfully made the same discoveries, and proved the truth of his assertions and the exactness of his demonstrations."

HIEROGLYPHICAL RESEARCHES.

Young's capacity and acquirements in regard to languages have been already glanced at. As a classical scholar his reputation was very high, and his Greek calligraphy was held to vie in elegance with that of Porson. A man so rounded in his culture could hardly be said to have an intellectual bent; but if he had one, the examination and elucidation of ancient manuscripts must have fallen in with it. It is quite possible, however, that, had he not been disheartened by the apparent success of Brougham, he would have clung more steadfastly to physical science. However this may be, we now find him in a new field. In October 1752 the first rolls of the papyri of Herculaneum, wearing the aspect of blackened roots, were discovered in what appeared to be the library of a palace near Portici. They had been covered to a depth of 120 feet with the mixed ashes, sand, and lava of Vesuvius. The inscriptions were for the most part written in Greek, but some of them were in Latin. The leaves were carbonised and hard, being glued together by heat to an almost homogeneous mass.

Learned Italians—Father Antonio, a writer of the Vatican, in particular—had devoted great labour and ingenuity to the separating of the leaves and the deciphering of the inscriptions. To the credit of the Prince of Wales, afterwards George IV., let it be recorded that he manifested from the first an enlightened, a liberal, and truly practical interest in these researches. He wrote to the Neapolitan Government, offering to defray all the expenses of unrolling and deciphering the papyri; and he sent out Mr. Hayter, a classical scholar of repute, to act as co-director with Rossini in the superintendence of the work. Mr. Hayter appears to have been unequal to the task committed to him. His translations were defective; his *lacunæ* serious and numerous, and he finally abandoned the manuscripts when he fled from Naples, with the Royal family, on the French invasion in 1806. Some of the rolls, which had been presented to the Prince of Wales, were committed to the care of the Royal Society, and placed by the Society in the hands of Dr. Young. He spent many months in devising and applying means for the opening of the leaves; and, though only partially successful in this respect,* he was able to correct many important errors, and to fill many serious gaps in the work of his predecessors.

The 'Quarterly Review' was established in 1809, and Young was intimate with its leading contributors. One of these, George Ellis, "a man of ardent affections," had resented, almost as personal to himself, the attacks on Young in the 'Edinburgh Review,' and Young's pen was soon invoked to enrich and adorn the pages of its rival. A great work, the Herculanensia, had been published, containing learned dissertations by the Rev. Robert Walpole, Sir William Drummond, and others, on the ancient condition of Herculaneum and its neighbourhood. The review of this work was committed to Young, and his article upon it, embodying his own views and researches, was published in 1810. "The appearance of the article," says Peacock, "equally remarkable for its critical acuteness and vigorous writing, at once placed its author, in the estimation of the public, in the first class of the scholars of the age." Gifford, the editor of the 'Quarterly,' described the article as "certainly beyond all praise." Ellis, at the same time, wrote thus to Young:—"It is a consolation to know that Brougham, who took advantage of the growing circulation of the 'Edinburgh Review' to disseminate his vile abuse of you, and Jeffrey, who permitted him to do so, should be condemned to hear your praises upon all sides." The tide had clearly turned in

---

* Davy afterwards tried his hand upon the rolls, with imperfect success.

Young's favour, even prior to his final and triumphant vindication by Fresnel.

From this time forward inscriptions of all kinds were sent to Young for discussion or interpretation. They were found in numbers among his papers after his death.*

It was a mind thus endowed and thus disciplined that now turned to the task of deciphering the hieroglyphics of Egypt. The more immediate cause of his grappling with this formidable but fascinating subject was the finding by Sir W. Rouse Boughton, in a mummy-case at Thebes, of a papyrus in Egyptian running hand, fragments of which, after serious injury to the manuscript, fell into the hands of Young. To Sir W. Boughton's communication to the Antiquarian Society, Young appended a short notice, the chief significance of which is the relation in which it stands to his subsequent researches. An adumbration of these, which must, under the circumstances, be weak and faint, I will endeavour to bring before you.

The famous Rosetta stone was discovered by the French in Egypt in 1799. It bore three inscriptions: the first, hieroglyphical or sacred; the second Enchorial †—a name given by Young to the common language employed by the Egyptians in the time of the Ptolemies; and the third Greek. At the end are given the following directions:—"What is here decreed shall be engraved on a block of hard stone, in sacred, in native, and in Greek characters, and placed in each temple of the first and second and third gods." All three inscriptions were more or less mutilated and effaced when the stone was discovered. Porson and Heyne had, however, succeeded in almost completely restoring the Greek one.

---

* "In the 19th volume of the 'Archæologia,'" says Young's biographer, "we find an interesting notice of a fragment of a very ancient papyrus, as well as several curious but somewhat barbarous sepulchral inscriptions of a late age from Nubia, which were submitted to him by Lord Mountnorris. In the Appendix to Captain's Light's Travels in Egypt, Nubia, Palestine, and Cyprus, he furnished translations and restorations of several Greek inscriptions; and when Barrow gave an account in the 'Quarterly Review' of recent researches in Egypt, more especially those of Caviglia on the Great Sphinx, it was from Young that he obtained the restoration of the inscription on the second digit of the great paw." In the 3rd volume of Young's Works, this inscription, taken from the 19th volume of the 'Quarterly Review,' is given, with translations into modern Greek, Latin, and English. The last-mentioned runs thus:—

"Thy form stupendous here the gods have placed,
  Sparing each spot of harvest-bearing land;
And with this mighty work of art have graced
  A rocky isle, encumber'd once with sand;
  And near the Pyramids have bid thee stand:
Not that fierce Sphinx that Thebes erewhile laid waste,
  But great Latona's servant mild and bland;
Watching that prince beloved who fills the throne
Of Egypt's plains, and calls the Nile his own.
That heavenly monarch [who his foes defies],
Like Vulcan powerful [and like Pallas wise]."

† Called in the Greek "ENCHORIA GRAMMATA," or letters of the country. Young deprecates the introduction, afterwards, by Champollion, of the term "DEMOTIC," or popular.

It had been a custom with Young to pay an annual visit to Worthing, and to pursue there for a portion of the year his practice as a physician. The Society of Antiquaries had caused copies of the three inscriptions of the Rosetta stone to be made and published, and in the summer of 1814, Young took all of them to Worthing, where he subjected them to a severe comparative examination. Baron Sylvestre de Sacy, an eminent orientalist, had discovered in the native Egyptian, certain groups of characters answering to proper names, while Akerblad, a profound Coptic scholar, had not only added to the number, but attempted to establish an alphabet answering to the native Egyptian inscription. Young took up the researches of these distinguished men as far as they could be relied on. Assuming all three inscriptions to express the same decree, one of them being in a language known to scholars, it was inferred by Young that a strict comparison of line with line, word with word, and character with character, would lead him by the sure method of science from the known to the unknown. He rapidly passed his predecessors. De Sacy had determined three proper names in the Egyptian; Akerblad nine others, and five or six Coptic words; while Young soon after detected the rudiments of fifty or sixty Coptic words, which, however, formed but a very small fraction of the whole inscription. And here an unexpected stumbling-block was encountered. The effort of Akerblad to reduce the whole Enchorial inscription to Coptic had failed,* and it soon became evident to Young that every such attempt must of necessity fail. His conviction and its grounds are first mentioned in a letter to Mr. Gurney, written in August 1814. "I doubt," he writes, "if it will be ever possible to reduce much more of it to Coptic, especially *as I have fully ascertained that some of the characters are hieroglyphics*." As bearing upon the *derivative origin* of the Enchorial inscription, the discovery here announced is of the highest importance. Young continues: "I have, however, made out the sense of the whole sufficiently for my purpose, and by means of variations from the Greek, I have been able to effect a comparison with the hieroglyphics which it would have been impossible to do satisfactorily without this intermediate step." In a letter to the Archduke John of Austria, dated 2nd August, 1816, Young announced that he had "now fully demonstrated the hieroglyphical origin" of the Enchorial inscription.†

---

* "Notwithstanding this failure, his name," says Peacock, "should ever be held in honour as one of the founders of our knowledge of Egyptian literature, to the investigation of which he brought no small amount of patient labour and philological learning."

† "The same discovery," says the editor of the third volume of Young's Works, "was announced by M. Champollion, as his own, in his memoir 'De

"I had thought it necessary," says Young, in an essay written to clear the air on this and various other points some years afterwards, "to make myself in some measure familiar with the remains of the old Egyptian language as they are preserved in the Coptic and Thebaic versions of the Scriptures; and I had hoped, with the assistance of this knowledge, to be able to find an alphabet which would enable me to read the Enchorial inscription, at least into a kindred dialect. But in the progress of the investigation I had gradually been compelled to abandon this expectation, and to admit the conviction that no such alphabet would ever be discovered, because it had never been in existence.

"I was led to this conclusion, not only by the untractable nature of the inscription itself, which might have depended on my own want of information and address, but still more decidedly by the manifest occurrence of a multitude of characters which were obviously imperfect imitations of the more intelligible pictures that were observable among the distinct hieroglyphics of the first inscription, such as a Priest, a Statue, a Mattock, or Plough, which were evidently, in their primitive state, delineations of the objects intended to be denoted by them, and which were, as evidently, introduced among the Enchorial characters."

Young, as we have seen, had begun his labours on the Rosetta stone in May 1814, and in the month of August he was able to announce to Mr. Gurney his discovery that some of the Enchorial characters were hieroglyphics. Prior to Young, no human being had dreamt of the transfer of the characters of the first inscription to the second. The first was pictural and symbolic; the second, to all appearance, a purely alphabetical running hand. It had always been regarded in this light. By means of the funeral papyri Young still further established the relationship between the first and second inscriptions. In 1816 he obtained from Mr. William Hamilton a loan of the noble work entitled 'Description de l'Egypte,' in which were carefully published several of the papyrus manuscripts. Many of the inscriptions dealt with the same text, and by comparing them one with another Young was able to trace the gradual departure from the original hieroglyphic characters. Probably with a view to more rapid writing, these had passed through various phases of degradation,

---

l'Ecriture Hiératique des Anciens Égyptiens,' published at Grenoble in 1821. . . . . This memoir contained several plates in which the hieroglyphic and hieratic characters are compared, on the same plan as Dr. Young's specimens in the 'Encyclopedia Britannica,' published in 1819. He sent a copy of them to Dr. Young, but withheld the letterpress. Dr. Young accordingly remained for several years under the impression that this work had been published at a much earlier period." Writing to Sir William Gell in 1827, in reference to this point, Young remarks, "I never knew till now how much later his publication was, for he gave it to me without the text." The publication was Champollion's 'Comparative Table of Hieroglyphics,' "containing," says Young, "what I had published in 1816," five years earlier.

until they reached the stage corresponding to the Enchorial inscription of the Rosetta stone, "which," says Young, "resembled in its general appearance the most unpicturesque of these manuscripts." Long before the time of Young, learned men had tried their hands on the Rosetta characters, but no relationship like that here indicated had ever been discerned.

Pre-eminent among the Egyptologists of that time was the celebrated Champollion, librarian at Grenoble. In his very first reference to Champollion, Dean Peacock speaks thus of the illustrious Frenchman:—"He had made the history, the topography, and antiquities of Egypt, as well as the Coptic language and its kindred dialects, the study of his life, and he started therefore upon this inquiry with advantages that probably no other person possessed; and no one who is acquainted with his later writings can call in doubt his extraordinary sagacity in bringing to bear upon every subject connected with it, not merely the most apposite, but also the most remote, and sometimes the most unexpected, illustrations." Thus equipped, however, Champollion made next to no progress before the advent of Young. "With the exception," says Peacock, "of the identification of a few additional Coptic words, very ingeniously elicited from the Egyptian text, he made no important advance on what had already been done by Akerblad. Like him, also, he abandoned the task of identifying the hieroglyphical inscription, or portions of it, with those corresponding to them in the Egyptian or Greek text, as altogether hopeless, in consequence of the very extensive mutilations which it had undergone."

Young, however, had determined about 90 or 100 characters of the mutilated hieroglyphic inscription (the funeral papyri enabled him afterwards to more than double the number), and these sufficed to prove, "first, that many simple objects were represented by their actual delineations; secondly, that many other objects, represented graphically, were used in a figurative sense only, while a great number of the symbols, in frequent use, could be considered as the pictures of no existing objects whatever; thirdly, that a dual was denoted by a repetition of the character, but that three characters of the same kind following each other implied an indefinite plurality, more compendiously represented by three lines or bars attached to a single character; fourthly, that definite numbers were expressed by dashes for units, and arches, either round or square, for tens; fifthly, that all hieroglyphic inscriptions were read from front to rear, as the objects naturally follow each other; sixthly, that proper names were included by the oval ring, or border, or *cartouche;** and seventhly, that the name of Ptolemy alone existed on this pillar, having only been completely identified by help of the analysis of the Enchorial inscription. And," adds Young, "as far as I have ever heard or read, *not one* of these particulars had ever been established and placed on record by *any other person*, dead or alive."

---

* Young's editor adds here, "The discovery was long afterwards made by Champollion that the cartouches were confined to the names of royal personages."

No man was a better judge of intellectual labour than Dean Peacock. The whole of Young's writings, preparatory and otherwise, were before him when he wrote; and he states emphatically, that it is impossible to estimate either the vast extent to which Dr. Young had carried his hieroglyphical investigations or the progress which he had made in them, without an inspection of these manuscripts. In reference to an article entitled "Egypt," written by Young in 1818, and published in the 'Encyclopædia Britannica' for 1819, a writer in the 'Edinburgh Review' for 1826 delivers the following weighty opinion: "We do not hesitate to pronounce this article the greatest effort of scholarship and ingenuity of which modern literature can boast." Even to an outsider it offers proof of astonishing learning and research. Still, Peacock assures us that this publication of 1819 could hardly be considered more than a popular and superficial sketch of the vast mass of materials on which it was founded.

Young was limited to what Peacock here calls "a popular and superficial sketch," by the fact that the article in the 'Encyclopædia Britannica' was written for ordinary readers rather than for critics or learned men. In this article, however, we are allowed a glimpse of Young's mode of collating and comparing the different inscriptions. He looks at the Enchorial inscription, and notices certain recurrent groups of characters; he looks at the Greek inscription, and finds there words with the same, or approximately the same, periods of recurrence. Thus, "a small group of characters occurring very often, in almost every line, might be either some termination or some very common particle; it must therefore be reserved till it is found in some decisive situation, after some other words have been identified, and it will then easily be shown to mean *and*. The next remarkable collection of characters is repeated twenty-nine or thirty times in the Enchorial inscription; and we find nothing that occurs so often in the Greek except the word *king*. . . . A fourth assemblage of characters is found fourteen times in the Enchorial inscription, agreeing sufficiently well in frequency with the name of *Ptolemy*. . . . By a similar comparison, the name of Egypt is identified. . . . Having thus," says Young, "obtained a sufficient number of common points of subdivision, we next proceed to write the Greek text over the Enchorial in such a manner that the passages ascertained may all coincide as nearly as possible; and it is obvious that the intermediate parts of each inscription will then stand very near to the corresponding passages of the other. . . . By pursuing the comparison of the inscriptions thus arranged, we ultimately discover the signification of the greater part of the individual Enchorial words."

Having thus compared the Greek text with the Enchorial, Young next proceeded to compare the Enchorial with the hieroglyphical. About half the lines of the latter were obliterated, and the rest were considerably defaced. Towards the ends, however, both inscriptions were fairly well preserved; and these were the portions subjected to the scrutiny of Young. Making allowance for the differences of space occupied by the two inscriptions, and measuring from the final words of the inscriptions, proportional distances, determined by the Enchorial characters for *God*, *King*, *Priest*, and *Shrine*, the meaning of which had been well established, Young sought at the places indicated by these measurements for the corresponding hieroglyphics. He soon found that *shrine* and *priest* were denoted by pictures of the things themselves. The other terms, *God* and *King*, were still more easily ascertained, from their situation near the name of Ptolemy. Having thus fixed his points of orientation, Young placed them side by side, and subjected the characters lying between them to a searching comparison. He offers in his article of 1819, the last line of the sacred characters, with the corresponding parts of the other inscriptions, as a "fair specimen of the result that has been attained from these operations."

Up to the time of which we now speak, although profoundly learned men had attempted to decipher the funeral papyri of Egypt, if we omit the labours of Young, very little progress had been made even in this direction; while in regard to the decipherment of the hieroglyphics nothing had been done. To Young "belongs the honour of having within a short space of time, discovered that the Enchorial writings contained symbolic as well as phonetic signs, and that the hieroglyphic inscriptions possessed not only a symbolic but a phonetic element. The latter discovery was based chiefly upon his analysis of the names of Ptolemy and Berenice."

A vast extension of our knowledge of Egyptian writing is to be ascribed to a circumstance which might almost be called miraculous. An Italian named Cassati, had brought to Paris several manuscripts from Upper Egypt. One was written exactly in the Enchorial character of the second inscription on the Rosetta stone. It was a deed of sale, and on the back of the manuscript was an endorsement in Greek. When in Paris, Young had received from Champollion a tracing of the Enchorial deed, but not of the Greek endorsement. About the same time, Mr. Grey, an English traveller, brought to England a number of manuscripts, which he placed in the hands of Dr. Young. One of them was written entirely in Greek, and Young immediately perceived that it was a perfect copy of the Enchorial deed of sale. He wrote immediately to Champollion, informing him of the fact, and begging him to send a copy of the Greek endorsement which he had omitted. This he did not do; but his countryman Raoul Rochette courteously and promptly responded to Young's request, and sent him a correct copy of the whole Cassati manuscript.

The possession of the Greek translation was of course an immense help to Young in his efforts to decipher the Enchorial deed, on which he was at this very time engaged. "I could not," he says, "but conclude that a most extraordinary chance had brought into my possession a document which was not very likely, in the first place, ever to have existed, still less to have been preserved uninjured for my information through a period of near two thousand years. But that this very extraordinary translation should have been brought safely to Europe, to England, and to me, at the very moment when it was most of all desirable to me to possess it, as the illustration of an original which I was then studying, but without any reasonable hope of being able to fully comprehend it,—this combination would in other times have been considered as affording evidence of my having become an Egyptian sorcerer."

Grey's manuscript related not to a sale of a house or field, but to portions of the collections and offerings made from time to time for the benefit of a certain number of mummies. The persons of whom the mummies were the remains were described at length in bad Greek, but though bad, a comparison between it and the Enchorial writing, gave the most important information regarding the orthography of ancient Egypt. Mr. Grey's collection contained three other similar deeds, all written in the Enchorial character of the Rosetta stone, and endorsed with the Greek registry. The dates of these documents closely corresponded with that of the Cassati manuscript which was 146 years before Christ. They refer to the sale of land, the boundaries of which were very clearly defined.* In those days, as we know, the Egyptians were the best land surveyors in the world. The comparison of these documents formed, as might be expected, an epoch in the history of Egyptian literature.

We now approach a period of stormy discussion regarding the claims of different discoverers. And as the tempest raged chiefly round Young and Champollion, it is desirable to fix with precision, if that be possible, the position of the learned Frenchman before he came into contact with Young. This, a work published by Champollion at Grenoble, in 1821, enables us to do. After speaking of the notions previously entertained regarding the hieroglyphical and epistolographic characters of the Egyptians, and of the opinion, universally diffused, that the Egyptian manuscripts, like those of to-day, are alphabetical, the author states his case thus:—"Une longue étude, et surtout une comparaison attentive des textes hiéroglyphiques avec ceux de la seconde espèce, regardés comme alphabétiques, nous ont conduit à une conclusion contraire.

---

* And the persons concerned equally well defined. In this respect the Egyptians might vie with the writers of Continental passports. The following is a translation of the famous papyrus of Anastasy, recording a deed of sale:—"There was sold by Pamonthes, aged about forty-five, of middle size, dark complexion, and handsome figure, bald, round faced, and straight nosed; by Snachomneus, aged about twenty, of middle size, sallow complexion, likewise round faced and straight nosed; and by Semmuthis Persineï, aged about twenty-two, of middle size, sallow complexion, round faced, flat nosed, and of quiet demeanour; and by Tathlyt Persineï, aged about thirty, of middle size, sallow complexion, round face, and straight nose, with their principal Pamonthes, a party in the sale; the four being of the children of Petepsais of the leather cutters of the Memnonia; out of the piece of level ground which belongs to them in the southern part of the Memnonia, eight thousand cubits of open field. . . . It was bought by Nechutes the less, the son of Asos, aged about forty, of middle size, sallow complexion, cheerful countenance, long face, and straight nose, with a scar upon the middle of his forehead, for 601 pieces of brass, the sellers standing as brokers, and as securities for the validity of the sale. It was accepted by Nechutes the purchaser."

Il résulte, en effet, de nos rapprochements:—

1° Que l'écriture des manuscrits Egyptiens de la seconde espèce (l'hiératique) n'est point alphabétique;

2° Que ce second système n'est qu'une simple modification du système hiéroglyphique, et n'en diffère uniquement que par la forme des signes;

3° Que cette seconde espèce d'écriture est l'hiératique des auteurs Grecs, et doit être regardée comme une tachygraphie hiéroglyphique;

4° Enfin, que les caractères hiératiques sont DES SIGNES DE CHOSES, ET NON DES SIGNES DE SONS."

There is no mention here of the name of Young, though he had, many years previously, made known to the world, as the result of his own researches, the first, second, and third of these propositions. With regard to the fourth, it incontestibly proves, as maintained by both Klaproth and the Dean of Ely, "that at this epoch, Champollion had either formed no conception of the existence of phonetic hieroglyphics, or had given it up as altogether untenable, if he had once entertained it." Immediately after the publication of this work in 1821, Champollion became acquainted with the "popular and superficial sketch"—in reality the transcendently able article of Young—published in the 'Encyclopædia Britannica' for 1819.

Peacock's analysis of what next occurred is not agreeable reading. Champollion's memoir of 1821 was rapidly suppressed, and soon became so scarce that it has been passed over by almost every author who has written on the subject. In the following year Champollion addressed a letter to M. Dacier, in which, to use the language of Peacock, we suddenly find him pushed forward into the inmost recesses of the sanctuary, reached by Young five years before. The plates, moreover, of the suppressed memoir were circulated, without dates and without letterpress. A copy of these plates was given by Champollion to Young, who was left in entire ignorance of the date of publication. "The suppression of a work," writes Peacock, in strong reproof, "expressing opinions which its author has subsequently found reason to abandon, may sometimes be excused, but rarely altogether *justified; but under no circumstances can such a justification be pleaded, when the suppression is either designed or calculated to compromise the claims of other persons with reference to our own.* The memoir in question very clearly showed that so late as the year 1821, Champollion had made no real progress in removing the mysterious veil which had so long enveloped the ancient literature of Egypt. The article "Egypt," written by Young, had meantime confessedly come under his observation. He saw the errors of his views and suppressed them, without giving due credit to the man who had first struck into the true path." In reference to an account given by Champollion of the labours of Young, Peacock remarks, "It would be difficult to point out in the history of literature a more flagrant example of the disingenuous suppression of the real facts bearing upon an important discovery."

And yet the Dean of Ely is by no means stingy in his praise of Champollion. It would be unjust, he says, to refuse to Champollion the honour due to his rare skill and sagacity, not merely in the application of a principle already known, but in its rapid extension to a multitude of other cases, so as not merely to point out its character and use, but also to determine the principal elements of a phonetic alphabet. His long-continued studies, Peacock remarks, had fitted him more than any other living man, Young himself hardly excepted, to deal with this subject, "and the rapidity of his progress, when once fully started on his career of discovery, was worthy of the highest admiration." Peacock, moreover, describes his work as ever memorable in the history of hieroglyphical research, not only from the vast range of knowledge which it displays, but from the clear and lucid order in which it is arranged. "It was," he continues, "singularly unfortunate that one who possessed so much of his own, should have been so much wanting in a proper sense of justice to those who had preceded him in these investigations, as materially to lessen his claims to the respect and reverence which would otherwise have been most willingly conceded to him."

With regard to the lack of literary candour, thus so strongly commented on, it is of interest to note the views concerning Champollion held by one of his own countrymen. Soon after the researches of Young had begun, an extremely interesting correspondence was established between him and De Sacy. As early as October 1814, Young was able to submit to his correspondent a "conjectural translation" into Latin of the Egyptian Rosetta inscription. He subsequently sent him an English translation, the receipt of which is acknowledged by De Sacy in a letter dated Paris, 20th July, 1815. The opening paragraph of this letter contains an allusion of considerable historic importance:—"Outre la traduction latine de l'inscription égyptienne, que vous m'avez communiquée, j'ai reçu postérieurement une autre traduction anglaise imprimée, que je n'ai pas en ce moment sous les yeux, *l'ayant prêté à M. Champollion sur la demande, que son frère m'en a faite d'après une lettre qu'il m'a dit avoir reçu de vous.*" In view of the statement of Champollion in a *Précis* of his researches published in 1824, that he had arrived at results similar to those obtained by Dr. Young, without having any knowledge of Young's opinions, the foregoing extract is significant. De Sacy goes on to recognise formally the progress which had been made by Young at the date of the foregoing letter. He asks some questions regarding Young's method, which in certain cases appeared to him enigmatical. The requisite explanations were promptly given by Young. In a labour of the kind here under consideration, that force of genius which we vaguely term *intuition* must come conspicuously into play; and it is not always easy for him to whom the exercise of this force is habitual to make plain to others the nature and results of its action.

De Sacy embodies in the letter above quoted some personal remarks which, were it not that their omission would involve a virtual injustice to Young, one would willingly pass over. "Si j'ai un conseil à vous donner," writes the Baron, "c'est de ne pas trop communiquer vos découvertes à M. Champollion. Il se pourrait faire qu'il prétendât ensuite à la priorité. Il cherche en plusieurs endroits de son ouvrage à faire croire qu'il a découvert beaucoup des mots de l'inscription égyptienne de Rosette. J'ai bien peur que ce ne soit là que du charlatanisme: j'ajoute même que j'ai de fortes raisons de le penser." The work of Champollion here referred to was entitled 'L'Égypte sous les Pharaons, ou recherches sur la géographie, la religion, la langue, les écritures, et l'histoire de l'Égypte avant l'invasion de Cambyses.' Two volumes of the work were published in 1814, but it was never completed.

In a letter written towards the end of 1815 Young passes the following judgment upon this book in regard to its relation to the Rosetta inscriptions:—

"I have only spent literally five minutes in looking over Champollion, turning, by means of the index, to the parts where he has quoted the inscription of Rosetta. He follows Akerblad blindly, with scarcely any acknowledgment. But he certainly has picked out the sense of a few passages in the inscription by means of Akerblad's investigations—although in four or five Coptic words which he pretends to have found in it, he is wrong in all but one—and that is a very short and a very obvious one. *My translation is printed; it is anonymous, and must for some time remain so; but everybody whose approbation is worth having will know the author.*"

Our neighbours, the French, have been always fond, perhaps rightly fond, of national glory, not only in military matters, but also in science and literature. They rallied round Champollion. Even De Sacy, who had previously warned Young against him, eventually joined in the general pæan. Arago also, who, in regard to the optical discoveries of Young had behaved so honourably, delivered an Eloge of Young, founded, according to Peacock, on the most imperfect and narrow views of the case. In fact, patriotism came into play, where cosmopolitanism ought to have been supreme. Arago seeks to make out that Young stands in the same relation to Champollion as Hooke, in regard to the doctrine of interferences, stands to Young. This is certainly a bold comparison. If, as observed by Young's editor, Arago had gone as far back as Zoega, the comparison might have been just. Arago himself gives his reasons for entering the controversy, and they are these:—"que l'interprétation des hiéroglyphes égyptiens est l'une des plus belles découvertes de notre siècle; que Young a lui-même mêlé mon nom aux discussions dont elle a été l'objet; qu'examiner enfin, si la France peut prétendre à ce nouveau titre de gloire, c'est agrandir la mission que je remplis en ce moment, c'est faire acte de bon citoyen. Je sais d'avance tout ce qu'on trouvera d'étroit dans ces sentimens; je n'ignore pas que le cosmopolitisme a son beau côté; mais en vérité, de quel nom ne pourrais-je pas le stigmatiser si lorsque toutes les nations voisines énumèrent avec bonheur les découvertes de leurs enfans, il m'était interdit de chercher dans cette enceinte même parmi des confrères dont je ne me permettrai pas de blesser la modesté, la preuve que la France n'est pas dégénérée, qu'elle aussi apporte chaque année son glorieux contingent dans le vaste dépôt des connaissances humaines."

The Copley medal of the Royal Society of London had been awarded to Arago in 1825, and on the 30th of November of that year, on handing over the medal to the gentleman deputed to receive it, Sir Humphry Davy, then President of the Society, had used the following words:—"Fortunately science, like that nature to which it belongs, is neither limited by time nor by space. It belongs to the world, and is of no country and no age." I do not hesitate to say that I prefer the sentiment of Davy to that of Arago.

Still, even in France, Young did not lack defenders. M. de Paravey, Inspecteur de l'Ecole Royale Polytechnique, for example, speaking of himself in the third person, makes the following remarks in a letter dated February 1835, six years after Young's death:—"Il y admira la science avec laquelle M. le Docteur Young avait rétabli la chronologie des Rois d'Egypte, ne commençant leur série qu'à la XVIII dynastie de Manethon, en regardant les séries antérieures comme inadmissibles; résultats auxquels des travaux tout différents avoient également conduit M. de Paravey: et, en outre, il jugea, et il juge encore, que le premier il entroit d'une manière plausible et sûre dans l'interprétation des hiéroglyphes, *fournissant ainsi à M. Champollion le jeune une clef sans laquelle ce dernier n'auroit jamais pu arriver aux résultats importants et curieux que depuis il a obtenu.*"

In the same sense, and almost in the same words, writes Sir Gardner Wilkinson, an ardent admirer of Champollion, and his chivalrous defender against the assaults made upon him after his death. After speaking of him as the kindler into a flame of the spark obtained by Young, he continues thus:—"Had Champollion been disposed to give more credit to the value and originality of Dr. Young's researches, and to admit that the real discovery of the *key* to the hieroglyphics, which in his dexterous hand proved so useful in unlocking those treasures, was the result of his [Young's] labours, he would unquestionably have increased his own reputation, without making any sacrifice." In another place, Wilkinson remarks, in regard to the reading of the hieroglyphics, "that Dr. Young gave the first idea and proof of their alphabetic force, which was even for some time after doubted by Champollion."

Peacock speaks with wondering admiration of the modesty and forbearance which he invariably showed in regard to Champollion. He complained a little, but he throws no doubt or insinuation upon the Frenchman's honour. He confines himself exclusively to his published writings, and makes no reference to the loads of labour which lay upon his shelves unpublished. Peacock complains, and justly complains, of the unfairness of comparing the Champollion of 1824 with the Young of 1816. Young was the initiatory genius. He gave Champollion the key, which he used subsequently with that masterly skill and sagacity which have rendered his name illustrious. But Peacock emphatically affirms that, while Champollion passed over Young's special researches in connection with the papyri of Grey and Cassati, he affirms with equal emphasis that whatever principle of discovery had been perceived and established or made known, is appropriated without acknowledgment, and the dates which would have proved the unquestionable priority of Dr. Young are carefully suppressed. No opportunity is lost of bringing prominently before the reader whatever error he may have committed, with a view of showing not only his (Champollion's) own superiority, but his entire independence and originality.

The Dean of Ely obviously felt very sore in regard to the treatment of Dr. Young. "It is not our object," he says, "to underrate the merits of the great contributions which were made by Champollion to our knowledge of hieroglyphical literature, but to protest against the persevering injustice with which he treated the labours of Dr. Young; and we feel more especially called upon to do so in consequence of finding that an author like Bunsen, occupying so high a position among men of letters, should have supported with the weight of his authority some of the grossest of his misrepresentations." Peacock acknowledges his own obligations to the valuable labours of his friend Mr. Leitch; but he also claims to have pursued an independent course by consulting the unpublished documents in his possession, which were unknown even to himself, until he was compelled to study them in connection with the publications which had been founded upon them. "It was only," he says, "after this perusal that I became fully aware how imperfectly the published writings of Dr. Young represented either the extent or the character of his researches; or the real progress he had made in the discovery of phonetic hieroglyphics many years before Champollion had made his appearance in the field."

The following Inscription, written by Mr. Hudson Gurney, was placed on a slab beneath the medallion by Sir Francis Chantrey in Westminster Abbey. The same inscription, somewhat modified, was placed on a marble slab in the village church of Farnborough, near Bromley, Kent, where the remains of Dr. Young were deposited in the vault of his wife's family:—

SACRED TO THE MEMORY OF
THOMAS YOUNG, M.D.,
FELLOW AND FOREIGN SECRETARY OF THE ROYAL SOCIETY,
MEMBER OF THE NATIONAL INSTITUTE OF FRANCE;
A MAN ALIKE EMINENT
IN ALMOST EVERY DEPARTMENT OF HUMAN LEARNING.
PATIENT OF UNINTERMITTED LABOUR,
ENDOWED WITH THE FACULTY OF INTUITIVE PERCEPTION,
WHO, BRINGING AN EQUAL MASTERY
TO THE MOST ABSTRUSE INVESTIGATIONS
OF LETTERS AND OF SCIENCE,
FIRST ESTABLISHED THE UNDULATORY THEORY OF LIGHT,
AND FIRST PENETRATED THE OBSCURITY
WHICH HAD VEILED FOR AGES
THE HIEROGLYPHICS OF EGYPT.

ENDEARED TO HIS FRIENDS BY HIS DOMESTIC VIRTUES,
HONOURED BY THE WORLD FOR HIS UNRIVALLED ACQUIREMENTS,
HE DIED IN THE HOPES OF THE RESURRECTION OF THE JUST.

BORN AT MILVERTON, IN SOMERSETSHIRE, JUNE 13TH, 1773,
DIED IN PARK SQUARE, LONDON, MAY 10TH, 1829,
IN THE 56TH YEAR OF HIS AGE.

GENERAL MONTHLY MEETING,

Monday, July 5, 1886.

WILLIAM HUGGINS, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

George A. Crawley, Esq.
The Right Hon. W. H. Smith, M.P.

were elected Members of the Royal Institution.

The Managers reported that they had re-appointed Professor James Dewar, M.A. F.R.S. as Fullerian Professor of Chemistry.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The French Government—Documents Inedits sur l'histoire de France: Melanges Historiques. Tome V. 4to. 1886.
Negociations Diplomatiques de la France avec la Toscane. Tome VI. 4to. 1886.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta; Rendiconti. Vol. II. Fasc. 10, 11. 8vo. 1886.
American Philosophical Society—Proceedings, No. 122. 8vo. 1886.
Antiquaries, Society of—Archæologia, Vol. XLIX. Part 2. 4to. 1886.
Astronomical Society, Royal—Monthly Notices, Vol. XLVI. No. 7. 8vo. 1886.
Bankers, Institute of—Journal, Vol. VII. Part 6. 8vo. 1886.
Bell, Robert, M.D. LL.D. (the Author)—The Forests of Canada. 8vo. 1886.
Mineral Resources of the Hudson's Bay Territories. 8vo. 1886.
Bernays, Albert J. Ph.D. F.C.S. M.R.I. (the Author)—Notes on Analytical Chemistry. 2nd Edition. 12mo. 1886.
British Architects, Royal Institute of—Proceedings, 1885–6, Nos. 17, 18. 4to.
British Museum (Natural History)—Catalogue of Birds. Vol. XI. 8vo. 1886.
Illustrations of Lepidoptera Heterocera. Part VI. 4to. 1886.
Catalogue of Fossil Mammalia. Part III. 8vo. 1886.
Introduction to the Study of Meteorites. 8vo. 1886.
Brymner, Douglas, Esq. (the Archivist)—Report on Canadian Archives, 1885. 8vo. 1886.
Chemical Society—Journal for June, 1886. 8vo.
Civil Engineers' Institution—Minutes of Proceedings, Vol. LXXXIV. 8vo. 1885–6.
Cornwall Polytechnic Society Royal—Fifty-third Annual Report. 8vo. 1885.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. VI. Part 3. 8vo. 1886.
East India Association—Journal, Vol. XVIII. No. 4. 8vo. 1886.
Eury, Lord (the Author)—The Laity and Church Reform. 8vo. 1886.

Editors—Amateur Photographer for June, 1886. 4to.
American Journal of Science for June, 1886. 8vo.
Analyst for June, 1886. 8vo.
Athenæum for June, 1886. 4to.
Chemical News for June, 1886. 4to.
Engineer for June, 1886. fol.
Engineering for June, 1886. fol.
Horological Journal for June, 1886. 8vo.
Iron for June, 1886. 4to.
Nature for June, 1886. 4to.
Revue Scientifique for June, 1886. 4to.
Telegraphic Journal for June, 1886. 8vo.
Zoophilist for June, 1886. 4to.
Franklin Institute—Journal, No. 726. 8vo. 1886.
General Medical Council—Second Report of Statistical Committee. 8vo. 1886.
Geographical Society, Royal—Proceedings, New Series, Vol. VIII. No. 6. 8vo. 1886.
Georgofili Reale Accademia—Atti, Quarta Serie. Vol. VIII. Disp. 4. Vol. IX. Disp. 1. 8vo. 1885–6.
Harlem, Société Hollandaise des Sciences—Archives Neerlandaises, Tome XX. Liv. 5. 8vo. 1886.
Liste Alphabétique de la Correspondance de Huygens. 4to. 1886.
Johns Hopkins University—Studies in Historical and Political Science, Fourth Series, No. 6. 8vo. 1886.
American Journal of Philology, No. 25. 8vo. 1886.
University Circular, Nos. 49, 50. 1886.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quarta, Vol. VI. No. 4. 8vo. And Disegni. fol. 1886.
Numismatic Society—Chronicle and Journal, 1886, Part 1. 8vo.
Pharmaceutical Society of Great Britain—Journal, June, 1886. 8vo.
Photographic Society—Journal, New Series, Vol. X. No. 8. 8vo. 1886.
Roberts-Austen, W. Chandler, Esq. F.R.S. M.R.I.—Annual Reports of the Deputy Master of the Mint, 1880–5. 8vo.
Royal Dublin Society—Transactions, Vol. III. Nos. 7–10. 4to. 1885.
Proceedings, Vol. IV. Parts 7–9; Vol. V. Parts 1, 2. 8vo. 1885–6.
Royal Society of London—Proceedings, No. 243. 8vo. 1886.
Smithsonian Institution, Washington—Annual Report for 1884. 8vo. 1885.
Society of Arts—Journal, June, 1886. 8vo.
Telegraph Engineers, Society of—Journal, No. 61. 8vo. 1886.
United Service Institution, Royal—Journal, No. 134. 8vo. 1886.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1886: Heft 5. 4to.
Vernon-Harcourt, L. F. Esq. M.A. (the Author)—Blasting Operations at Hell Gate, New York. (Inst. Civil Engrs. Proc. LXXXV.) 8vo. 1886.
Vienna—Naturhistorischen Hofmuseums—Annalen. Band I. No. 2. 4to. Wien 5, 1886.
Zoological Society—Proceedings, 1886, Part 1. 8vo.

GENERAL MONTHLY MEETING,

Monday, November 1, 1886.

Henry Pollock, Esq. Treasurer and Vice-President, in the Chair.

Charles Witham Herbert, Esq.
Arthur Lasenby Liberty, Esq.

were elected Members of the Royal Institution.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India. Records, Vol. XIX. Part 3.
 Memoirs: Palæontologia Indica. Ser. X. Vol. IV. Part 1.; Ser. XIV. Vol. I. Part 3, Fasc. 6. 4to. 1886.
The Secretary of State for India—Great Trigonometrical Survey of India. Synoptical, Vol. XIII. A. 4to. 1886.
The Lords of the Admiralty—Greenwich Observations for 1884. 4to. 1886.
 Greenwich Spectroscopic and Photographic Results, 1884. 4to. 1886.
 Observations of the Great Comet, 1882, II. 4to. 1886.
 Cape Meridian Observations, 1879–1881. 8vo.
Ministry of Public Works, Rome—Giornale del Genio Civile. Serie Quarta. Vol. VI. Nos. 5–6. 8vo. And Disegni. fol. 1886.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. Vol. II. Fasc. 12, 13, 14; Vol. II. 2^{o} Semestre, Fasc. 1, 2, 3, 4. 5, 6. 8vo. 1886.
 Memorie della Classe di Scienze Morali, Storiche e Filologiche. Serie 3^{a}, Vol. XII. 4to. 1884.
 Memorie della Classe di Scienze Fisiche, Matematiche e Naturali. Serie 3^{a}, Vols. XVIII. XIX.; Serie 4^{a}, Vol. II. 4to. 1884–5.
Academy of Natural Sciences, Philadelphia—Proceedings, 1886, Part 1. 8vo. 1886.
Amsterdam Royal Society of Zoology—Bijdragen tot de Dierkunde, Afl. 13. 4to. 1886.
Antiquaries, Society of—Proceedings, Vol. XI. Nos. 1, 2. 8vo. 1886.
Armagh Observatory—Second Armagh Catalogue of 3300 Stars for 1875. 8vo. 1886.
Asiatic Society of Bengal—Journal, Vol. LV. Part 1, Nos. 1, 2; Part 2, Nos. 1, 2. 8vo. 1886.
 Proceedings, 1886, Nos. 1–7. 8vo.
Asiatic Society, Royal—Journal, Vol. XVIII. Parts 3, 4. 8vo. 1886.
Astronomical Society, Royal—Monthly Notices, Vol. XLVI. Nos. 8, 9. 8vo. 1886.
Australian Museum, Sydney—Catalogue of Echinodermata, Part 1. 8vo. 1885.
Bankers, Institute of—Journal, Vol. VII. Part 7. 8vo. 1886.
Basel Naturforschende Gesellschaft—Verhandlungen, 8te Theil, Heft 1. 8vo. 1886.
Behnke, Emil, Esq. (the Author)—Physician and Voice Trainer. 8vo. 1886.
British Architects, Royal Institute of—Proceedings, 1885–6, No. 19; 1886–7, No. 1. 4to.
 Transactions, Vol. II. 4to. 1886.
 Kalendar, 1886. 8vo.

British Association for the Advancement of Science—Report of Meeting at Aberdeen, 1885. 8vo. 1886.
Canadian Economics. 8vo. 1885.
British Museum (Natural History)—Catalogue of the Blastoidea. 4to. 1886.
Guide to Galleries of Geology and Palæontology. 8vo. 1886.
Chemical Society—Journal for July-Oct. 1886. 8vo.
Chile, Officina Central Meteorolojica—Annuario, 1886. 8vo. Nos. 1, 2.
Civil Engineers' Institution—Minutes of Proceedings, Vols. LXXXV. LXXXVI. 8vo. 1885-6.
Clinical Society—Transactions, Vol. XIX. 8vo. 1886.
Cobden Club—G. W. Medley. Reciprocity Craze. 12mo. 1881.
Hon. G. C. Brodrick. Reform of English Land System. 12mo. 1883.
W. E. Baxter. Our Land Laws of the Past. 12mo. 1885.
Sir L. Mallet. National Income and Taxation. 12mo. 1885.
E. R. Pearce-Edgcumbe. Popular Fallacies regarding Trade and Foreign Duties. 12mo. 1885.
Sir R. Torrens. Transfer of Land by Registration. 12mo. 1885.
A. Mongredien. Trade Depression. 12mo. 1885.
———— Free Trade and English Commerce. 12mo. 1885.
———— Western Farmer of America. 12mo. 1886.
W. Birkmyre. The India Council. 12mo. 1885.
———— Secretary of State for India in Council. 12mo. 1886.
J. E. T. Rogers. Local Taxation. 12mo. 1886.
F. C. Montague. Old Poor Law and New Socialism. 12mo. 1886.
C. S. Salvin. The Crown Colonies of Great Britain. 12mo. 1886.
R. Gowing. Richard Cobden. 12mo. 1886.
Collins, Louis, Esq. (the Author)—The Advertisers' Guardian, No. 3. 8vo. 1886.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. VI. Parts 4, 5. 8vo. 1886.
Dax: Société de Borda—Bulletins, 2^e^ Serie, Onzième Année, 2^e^ and 3^e^ Trimestre. 8vo. 1886.
Devonshire Association for the Advancement of Science, Literature, and Art—Report and Transactions, Vol. XVIII. 8vo. 1886.
The Devonshire Domesday, Part 3. 8vo. 1886.
East India Association—Journal, Vol. XVIII. Nos. 5, 6. 8vo. 1886.
Edinburgh, Royal Observatory—Astronomical Observations, 1877-1886, Vol. XV. 4to. 1886.
Editors—American Journal of Science for July-Oct. 1886. 8vo.
Analyst for July-Oct. 1886. 8vo.
Athenæum for July-Oct. 1886. 4to.
Chemical News for July-Oct. 1886. 4to.
Chemist and Druggist for July-Oct. 1886. 8vo.
Engineer for July-Oct. 1886. fol.
Engineering for July-Oct. 1886. fol.
Horological Journal for July-Oct. 1886. 8vo.
Industries for July-Oct. 1886. fol.
Iron for July-Oct. 1886. 4to.
Nature for July-Oct. 1886. 4to.
Revue Scientifique for July-Oct. 1886. 4to.
Telegraphic Journal for July-Oct. 1886. 8vo.
Zoophilist for July-Oct. 1886. 4to.
Ellis, William, Esq. F.R.A.S. (the Author)—Brief Historical Account of the Barometer. (Journ. of Meteorological Society, XII.) 8vo. 1886.
Florence, Biblioteca Nazionale Centrale—Bolletino, Num. 1 to 15. 8vo. 1886.
Franklin Institute—Journal, Nos. 727, 728, 729. 8vo. 1886.
Geographical Society, Royal—Proceedings, New Series, Vol. VIII. Nos. 7-10. 8vo. 1886.
Geological Society—Quarterly Journal, No. 167. 8vo. 1886.
Geological Society of Ireland, Royal—Journal, Vol. XVII. Part 1. 8vo. 1886.

Glasgow Philosophical Society—Proceedings, Vol. XVII. 8vo. 1886.
Hamilton Association—Journal and Proceedings, Vol. I. Part 2. 8vo. 1885.
Harlem Société Hollandaise des Sciences—Archives Néerlandaises, Tome XXI. Liv. 1. 8vo. 1886.
Iron and Steel Institute—Journal, 1886, No. 1. 8vo.
Jablonowski'sche Gesellschaft, Leipzig, Furstliche—Preisschrift, No. 26. 8vo. 1886.
Johns Hopkins University—Studies in Historical and Political Science, Fourth Series, Nos. 7, 8, 9. 8vo. 1886.
University Circular, No. 51. 4to. 1886.
American Chemical Journal, Vol. VIII. No. 4. 8vo. 1886.
Linnean Society—Journal, Nos. 114, 115, 116, 145, 146, 147, 151. 8vo. 1886.
Transactions, 2nd Series: Zoology, Vol. II. Parts 12, 15, 16, 17; Vol. III. Part 4. 4to. 1885-6.
Liversidge, Professor A. F.R.S. (the Author)—Address to Royal Society of New South Wales. 8vo. 1886.
Madrid Royal Academy of Sciences—Revista: Tome XXI. Nos. 7, 8, 9; Tome XXII. No. 1. 8vo. 1886.
Manchester Steam Users' Association—Boiler Explosions Act, 1882. Board of Trade Reports, Nos. 87-129. fol. 1886.
Manila: Universidad de Sto Tomas—Discurso por M. L. Hernando. 4to. 1886.
Mechanical Engineers' Institution—Proceedings, 1886, No. 2. 8vo.
Medical and Chirurgical Society, Royal—Proceedings, New Series, No. 13. 8vo. 1886.
Transactions, Vol. LXIX. 8vo. 1886.
Meteorological Office—Monthly Weather Report for Feb. 1886. 4to.
Observations of the International Polar Expeditions, 1882-3. Fort Rae. 4to. 1886.
Meteorological Society, Royal—Quarterly Journal, No. 59. 8vo. 1886.
Meteorological Record, No. 21. 8vo. 1886.
Middlesex Hospital—Reports for 1884. 8vo. 1886.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXV. Part 3. 8vo. 1886.
Numismatic Society—Chronicle and Journal, 1886, Part 2. 8vo.
Odontological Society of Great Britain—Transactions, Vol. XVIII. No. 8. New Series. 8vo. 1886.
Perry, Rev. S. J. F.R.S. (the Author)—Results of Meteorological and Magnetical Observations, 1885. 12mo. 1886.
Pharmaceutical Society of Great Britain—Journal, July-Oct. 1886. 8vo.
Phipson, Dr. T. L. (the Author)—Outlines of a New Atomic Theory. (British Association.) 4to. 1886.
Photographic Society—Journal, New Series, Vol. X. No. 9; Vol. XI. No. 1. 8vo. 1886.
Physical Society of London—Proceedings, Vol. VIII. Part 1. 8vo. 1886.
Popoff, Constantine, Esq. (the Translator)—What I Believe. By Leon Tolstoi. 8vo. 1885.
Preussische Akademie der Wissenschaften—Sitzungberichte, I.-XXXIX. 8vo. 1886.
Radcliffe Observatory—Radcliffe Observations for 1883. 8vo. 1886.
Richardson, B. W. M.D. F.R.S. (the Author)—The Asclepiad, Vol. III. Nos. 11, 12. 8vo. 1886.
Royal College of Surgeons of England—Calendar, 1886. 8vo.
Royal Society of London—Philosophical Transactions, Vol. CLXXVI. Part 2. 4to. 1886.
Proceedings, Nos. 244, 245, 246. 8vo. 1886.
Saurbeck, A. Esq. (the Author)—Prices of Commodities and the Precious Metals. (Journ. of Statistical Society.) 8vo. 1886.
Saxon Society of Sciences, Royal—Philologisch-Historiche Classe: Berichte, 1886, No. 1. 8vo.
Mathematische-Physische Classe: Berichte, 1886. 8vo.

Seismological Society of Japan—Transactions, Vol. IX. Part 2. 8vo. 1886.
Society of Arts—Journal, July–Oct. 1886. 8vo.
Statistical Society—Journal, Vol. XLIX. Parts 2, 3. 8vo. 1886.
Catalogue of the Library. Index. 4to. 1886.
St. Petersbourg, Académie des Sciences—Mémoires, Tome XXXIII. Nos. 6, 7, 8; Tome XXXIV. Nos. 1–4. 4to. 1886.
Telegraph Engineers, Society of—Journal, Nos. 62, 63. 8vo. 1886.
Tokio University—Calender der Medicinischen Fakultät, 1883–4. 8vo. 1885.
United Service Institution, Royal—Journal, No. 135. 8vo. 1886.
United States Geological Survey—Bulletins, Nos. 24–26. 8vo. 1885.
Monograph IX. 4to. 1885.
Fifth Annual Report, 1883–4. 4to. 1885.
Upsal University—Bulletin Mensuel de l'Observatoire, Vol. XVII. 4to. 1885.
Nova Acta, Ser. III. Vol. XIII. Fasc. 1. 4to. 1886.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1886: Heft 6, 7. 4to.
Victoria Institute—Journal, No. 78. 8vo. 1886.
Victoria Royal Commissioners—Illustrated Handbook of Victoria. 4to. 1886.
Yorkshire Archæological and Topographical Association—Journal, Part 36. 8vo. 1886.
Zoological Society—Proceedings, 1886, Parts 2, 3. 8vo.
Transactions, Vol. XII. Part 3. 4to. 1886.

GENERAL MONTHLY MEETING,

Monday, December 6, 1886.

Henry Pollock, Esq. Treasurer and Vice-President, in the Chair.

Mrs. Lauder Brunton,
Thomas Buzzard, M.D. F.R.C.P.
The Right Hon. Lord Thurlow, F.R.S.
Mrs. Annie S. Tweedie,

were elected Members of the Royal Institution.

With reference to the report of the Managers read at the previous meeting, a Resolution was unanimously passed authorizing the Sale of a part of the New Three per Cent. Consols or Consols belonging to and standing in the name of the Royal Institution of Great Britain, sufficient to raise a sum of not exceeding £2000 cash.

The Special Thanks of the Members were returned to John P. Fearfield, Esq. *M.R.I.* for his valuable present of a Turning Lathe.

The following Lecture Arrangements were announced:

PROFESSOR DEWAR, M.A. F.R.S. *M.R.I.* Six Lectures (adapted to a Juvenile Auditory) on THE CHEMISTRY OF LIGHT AND PHOTOGRAPHY. On Dec. 28 (*Tuesday*), Dec. 30, 1886; Jan. 1, 4, 6, 8, 1887.

PROFESSOR ARTHUR GAMGEE, M.D. F.R.S. Eleven Lectures on THE FUNCTION OF RESPIRATION. On *Tuesdays*, Jan. 18 to March 29.

PROFESSOR A. W. RÜCKER, M.A. F.R.S. *M.R.I.* Five Lectures on MOLECULAR FORCES. On *Thursdays*, Jan. 20, 27, Feb. 3, 10, 17.

EDMUND GOSSE, Esq. M.A. Three Lectures on THE CRITICS OF THE AGE OF ANNE. On *Thursdays*, Feb. 24, March 3, 10.

PROFESSOR F. MAX MÜLLER, M.A. LL.D. Three Lectures on THE SCIENCE OF THOUGHT. On *Thursdays*, March 17, 24, 31,

CARL ARMBRUSTER, Esq. Five Lectures on MODERN COMPOSERS OF CLASSICAL SONG. On *Saturdays*, Jan. 22, 29, Feb. 5, 12, 19.

THE RIGHT HON. LORD RAYLEIGH, M.A. D.C.L. LL.D. F.R.S. *M.R.I.* Six Lectures on SOUND. On *Saturdays*, Feb. 26, March, 5, 12, 19, 26.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India. Records, Vol. XIX. Part 4. 8vo. 1886.
Memoirs: Palæontologia Indica. Ser. X. Vol. IV. Part 2. 4to. 1886.
The Corporation of the City of London—Descriptive Account of the Guildhall of the City of London. By J. E. Price. fol. 1886.
Agricultural Society of England, Royal—Journal, Second Series, Vol. XXII. Part 2. 8vo. 1886.
Bankers, Institute of—Journal, Vol. VII. Part 8. 8vo. 1886.
Birmingham Philosophical Society—Proceedings, Vol. V. Part 1. 8vo. 1886.
British Architects, Royal Institute of—Proceedings, 1886-7, Nos. 2, 3. 4to.
Chemical Society—Journal for Nov. 1886. 8vo.
Ebstein, Professor W. (the Author)—La Goutte: sa Nature et son Traitement. Traduction du E. Chambard. 8vo. Paris, 1886.
Editors—American Journal of Science for Nov. 1886. 8vo.
Analyst for Nov. 1886. 8vo.
Athenæum for Nov. 1886. 4to.
Chemical News for Nov. 1886. 4to.
Chemist and Druggist for Nov. 1886. 4to.
Chemists' and Druggists' Diary for 1887. 4to.
Engineer for Nov. 1886. fol.
Engineering for Nov. 1886. fol.
Horological Journal for Nov. 1886. 8vo.
Industries for Nov. 1886. fol.
Iron for Nov. 1886. 4to.
Nature for Nov. 1886. 4to.
Revue Scientifique for Nov. 1886. 4to.
Telegraphic Journal for Nov. 1886. 8vo.
Zoophilist for Nov. 1886. 4to.
Franklin Institute—Journal, No. 731. 8vo. 1886.
Geographical Society, Royal—Proceedings, New Series, Vol. VIII. No. 11. 8vo. 1886.
Supplementary Papers, Vol. I. Part 3. 8vo. 1886.

Geological Society—Quarterly Journal, No. 168. 8vo. 1886.
Georgofili Reale Accademia—Atti, Quarta Serie. Vol. IX. Disp. 2, 3. 8vo. 1886
Johns Hopkins University—Studies in Historical and Political Science, Fourth Series, No. 10. 8vo. 1886.
University Circular, No. 52. 4to. 1886.
American Chemical Journal, Vol. VIII. No. 5. 8vo. 1886
Lee, Henry, Esq. M.R.I. (the Author)—Tapetum Lucidum. (Medico-Chir. Trans. LXIX.) 8vo. 1886.
Linnean Society—Journal, No. 126. 8vo. 1886.
Maryland Medical and Chirurgical Faculty—Transactions, 1886. 8vo.
Mauritius Royal Society of Arts and Sciences—Transactions, Vols. XI. to XVIII. 8vo. 1883–6.
Meteorological Office—Quarterly Weather Report, 1878, Part 1. 4to. 1886.
Monthly Weather Report for June 1886. 4to. 1886.
Weekly Weather Report, Vol. III. Nos. 34 to 41. 4to. 1886.
Meteorological Society, Royal—Quarterly Journal, No. 60. 8vo. 1886.
Meteorological Record, No. 22. 8vo. 1886.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quarta, Vol. VI. Nos. 7, 8. 8vo. And Disegni. fol. 1886.
New South Wales Department of Mines—Annual Report for 1885. fol. 1886.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXV. Part 4. 8vo. 1886.
Odontological Society of Great Britain—Transactions, Vol. XIX. No. 1. New Series. 8vo. 1886.
Pharmaceutical Society of Great Britain—Journal, Nov. 1886. 8vo.
Physical Society of London—Proceedings, Vol. VIII. Part 2. 8vo. 1886.
Saxon Society of Sciences, Royal—Mathematisch-physische Classe: Abhandlungen, Band XIII. Nos. 6, 7. 8vo. 1886.
Society of Arts—Journal, Nov. 1886. 8vo.
St. Petersbourg, Academie des Sciences—Bulletin, Tome XXXI. No. 2. 4to. 1886.
Teyler Museum—Archives, Ser. II. Vol. II. 4e Partie. 4to. 1886.
Catalogue de la Bibliothèque, 3e, 4e, Liv. 4to. 1886.
United Service Institution, Royal—Journal, No. 136. 8vo. 1886.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1886. Heft 8. 4to.

INDEX TO VOLUME XI.

ABEL, Sir Frederick, Accidental Explosions by Non-explosive Liquids, 219.
Alps, Building of the, 53.
Anatomical and Medical Knowledge of Ancient Egypt, 378.
Anderson, W., New Applications of the Mechanical Properties of Cork, 436.
Animal Magnetism, 25.
Annual Meetings (1884) 84, (1885) 283, (1886) 467.
Ansdell, Gerrard, Researches on Meteorites, 541.
Antipyrine, 460.
Arago's Discovery in Induction, 123.
Arnold, Matthew, Emerson (*no abstract*), 43.

BENZENE and its Derivatives, 452.
Besant, Walter, Art of Fiction, 70.
Bichat's View of the Nervous System, 530.
Biblical Cities of Egypt, 384.
Bicycles, 13.
Bolometer, 273.
Bonney, Professor T. G., Building of the Alps, 53.
Boys, C. Vernon, Bicycles and Tricycles, 13.
Braid, Dr., and Mesmerism, 26, 35.
Bunsen's experiments on Dissociation, 474.
Busk, George, resigns Treasurership, 468.

CAILLETET Apparatus, 148.
Capillary Attraction, 483.
Carruthers, W., British Fossil Cycads, and their relation to Living Forms (*no abstract*), 283.
Chladni's Theory of Meteorites, 332.
Cholera: its Cause and Prevention, 288.
Christie, W. H. M., Universal Time, 387.
Coal Tar Industry, 450; Sources of Products, 451; Colours, 454; Antipyretic Medicines, 459; Aromatic Perfumes, 462; Saccharine, 462.
Cold: its Production, and Effects on Microphytes, 305.
Coleman, J. J., Mechanical Production of Cold, 305.
Colours, 107.
Common, A. A., Photography as an Aid to Astronomy, 367.
Cork, 437; Applications of its Mechanical Properties, 444.
Corona, Solar, 202; Photographed, 204.
Cotyledons, 517.
Critical Temperatures and Pressures of various Substances, 151.
Crystallisation, 508.

DARWINIAN Theory of Instinct, 131.
Deville's Experiments on Dissociation, 474.
Dewar, Professor, Liquefied Gases, 148.
—— Liquid Air and the Zero of Absolute Temperature (*no abstract*), 318.
—— The Story of a Meteorite, 328.
—— Researches on Meteorites, 541.
"Dhurmsala" Meteorite, 328; 543.
Dilatancy, 354.
Dissociation Temperatures, 471.
"Doterel" Explosion, 227.
Dust and Smoke, 520.

"ECONOMISER" for Grates, 342.
Egypt: Anatomy and Medicine, 378.
Egypt Exploration Fund, 384.
Electrical Deposition of Dust and Smoke, 520.
Energy defined, 571.
Explosions by Non-explosive Liquids, 218.

FARADAY'S Researches on Induction, 119.
Fauna of the Seashore, 168.
Fiction, Art of, 70.
Films, 243.
Fireplace Construction, 338.
Fixed Stars, Distances of, 91.
Fleming, Sandford, Scheme for Universal Time, 390.
Flower, Professor W. H., The Wings of Birds, 364.
Fluids and Solid Metals, 395.
Frozen Meat, 308.

GARRICK as an Actor, 304.
Gas Furnaces, 471.

Gases, Liquefied, 148; Solidified, 550.
Gaskell, Dr. W. H., Sympathetic Nervous System, 530.
Gill, David, Distances of the Fixed Stars, 91.
Glass Manufacture, 413.
Granular Material, Properties of, 354.
Graphites, 515.
Grubb, Howard, Telescopic Objectives and Mirrors, 413.

HENDERSON'S Observations, 95.
Hieroglyphical Researches, 574.
Horsley, Victor. Motor Centres of the Brain, and the Mechanism of the Will. 250.
Huggins, Dr. W., Solar Corona, 202.
Hughes, Professor D. E., Theory of Magnetism, 1.
Hypnotism, 26.

INDIGO, Artificial, 459.
Indophenol, 457.
Induction, 119.
Inhibition of Nerve Centre, 31.
Instinct, Theory of, 131.

JOULE'S Explanation of the Heating of Meteorites, 333.
Judd, Professor J. W., Krakatoa, 85.

KAIRINE, 460.
Koch's, Dr., Cholera Investigation, 300.
Krakatoa, 85.

LAMP Explosions, 230.
Langley, J. N., Physiological Aspect of Mesmerism, 25.
—— S. P., Sunlight and the Earth's Atmosphere, 265.
Lankester, Professor Ray, Marine Biological Laboratory, 215.
Leaves, Forms of, 197.
Lick Observatory, 429.
Light in relation to Plants, 113.
Liquid Films, 243.
Living Contagia, 161.
Lodge, Professor Oliver, Electrical Deposition of Dust and Smoke, 520.
Lubbock, Sir John, Forms of Leaves, 197.
—— Forms of Seedlings, 517.

MACALISTER, Professor A., Anatomical and Medical Knowledge of Ancient Egypt, 378.
McKendrick, J. G., Effects of Cold upon Microphytes, 305.
Magnetism, Theory of, 1.
Magneto-Electric Induction, 119.
Marine Biological Laboratory, 215.
Mascart, Mons. E., Sur les Couleurs, 107.
Mechanism of the Will, 250.
Mesmerism, Physiological Aspect, 25.
Metals, 395.
Meteorites, 328.
Meyer's Experiments on Dissociation, 476.
Mirrors, Figuring of Plane, 429.
Mohammedan Mahdis, 147.
Monthly Meetings:—
  (1884) March, 11: April, 68; May, 88; June, 116; July, 153; November, 155; December, 159.
  (1885) February, 175; March, 216; April, 263; May, 285; June, 317; July, 319; November, 321; December, 325.
  (1886) February, 335; March, 376; April, 433; May, 468; June, 538; July, 589; November, 591; December, 594.
Moseley, Professor H. N., Fauna of the Seashore, 168.
Motion, 178; of Water, 44.
Motor Centres of the Brain, 250.

NAVILLE'S Discoveries in Egypt, 384.
Nervous Systems, 530.
Newton, Charles T., German Discoveries at Pergamus (*no abstract*), 217.

OBJECTIVES, 413; Calculation of Curves, 415; Measurement of Curves, 418; Flexure, 419; polishing, 421; Figuring and Testing, 424.
Odling, Professor W., Dissolved Oxygen of Water (*no abstract*), 90.
Optical Glass Testing, 413.
Otto Bicycle, 23.
Oxygen Liquefied, 148; Solidified, 550.

PARALLAXES of Stars, 96.
Pasteur's Researches, 161.
Pauer, Professor Ernst, Works of Living Composers for the Pianoforte, 171.
Petroleum, 234; Explosions, 222.
Pianoforte Music, 171.
Photography applied to Astronomy, 104, 367.
Plateau's Researches on Films, 243.
Pollock, W. H., Garrick as an Actor, 301.

Poole, R. S., Discovery of the Biblical Cities of Egypt, 384.

Rayleigh, Lord, Water Jets and Water Drops (*no abstract*), 284.
Reflex Action from the Cerebral Cortex, 29.
Regenerative Furnaces, 471.
Reynolds, Osborne, Two Manners of Motion of Water, 44.
—— Experiments showing Dilatancy, 354.
Roberts-Austen, Professor W. Chandler, Properties common to Fluids and Solid Metals, 395.
Romanes, G. J., Darwinian Theory of Instinct, 131.
Roscoe, Sir Henry E., Recent Progress in the Coal Tar Industry, 450.
Rucker, Professor A. W., Liquid Films, 243.
Rumford's Researches on Fuel and Heat, 338; Principles of Fireplace Construction, 344.

Saccharine, 462.
Sanderson, Dr. J. Burdon, Cholera: its Cause and Prevention, 288.
Seedlings, 517.
Sidereal Astronomy, Future Problems, 91.
Siemens, Frederick, Dissociation Temperatures, 471.
Smith, W. Robertson, Mohammedan Mahdis, 147.
—— Willoughby, Volta-Electric and Magneto-Electric Induction, 119.
Solar Corona, 202.
—— Energy, 279.
Spring's Researches on Metals, 405.
Stoney, G. Johnstone, How Thought presents itself in Nature, 178.
Sun, Colour of the, 265.
Sunlight and the Earth's Atmosphere, 265.
Sympathetic Nervous System, 530.

Teale, T. Pridgin, Principles of Domestic Fireplace Construction, 338.
Telescopic Objectives and Mirrors, 413.
Temperatures, 148, 471, 550.
Thalline, 460.
Thomson, J. Millar, Suspended Crystallisation, 508.
—— Sir William, Capillary Attraction, 483.
Thought in Nature, 178.
Time-reckoning 387.
Tricycles, 13.
Tyndall, Professor, on Living Contagia, 161.
—— on Thomas Young, 553.

Universal Time, 387.

Volcano of Krakatoa, 85.
Volta-Electric Induction, 119.

Water, Motion of, 44.
Wave Theory, 562.
Weldon, W. F. R., Adaptation to surroundings as a Factor in Animal Development (*no abstract*), 287.
Wheels moving round a Curve, 19.
Whitney, Mount, 275.
Wings of Birds, 364.
Wright's Researches on Meteorites, 541.

Young, Thomas, Early Life and Studies, 553; the Wave Theory, 562; Hieroglyphical Researches, 574.

END OF VOL. XI.

LONDON:
PRINTED BY WILLIAM CLOWES AND SONS, LIMITED,
STAMFORD STREET AND CHARING CROSS.

This book should be returned to the Library on or before the last date stamped below.

A fine of five cents a day is incurred by retaining it beyond the specified time.

Please return promptly.

DUE MAR 7 1916

~~DUE MAR 27 1916~~

DUE APR 13 1916

DUE MAY 12 1917

DUE MAY 16 1921

~~DUE OCT 25 '39~~

~~DUE OCT 25 '39~~

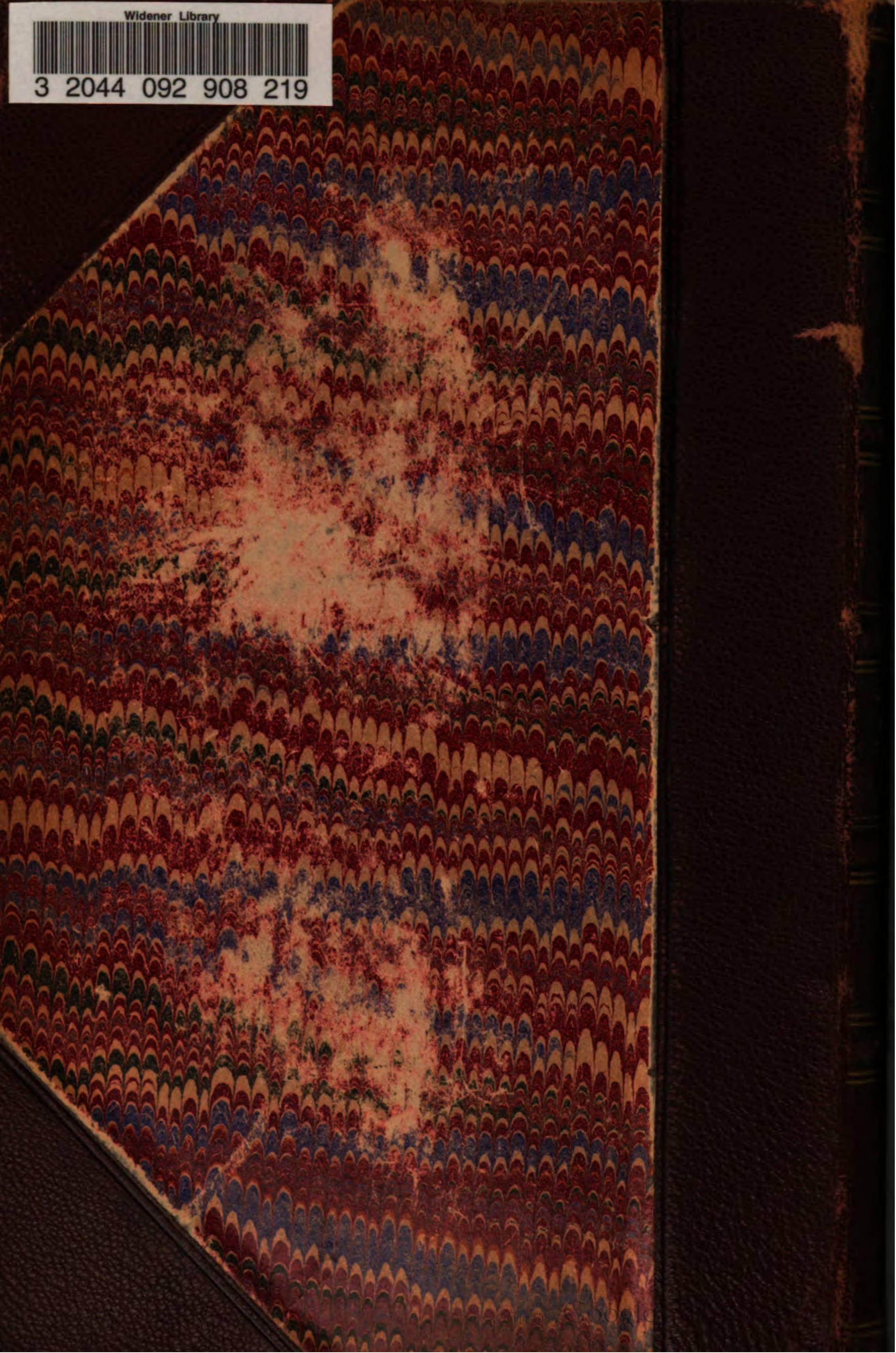
~~DUE DEC 4 '42~~

~~DUE DEC 4 '42~~

OCT 10 1973

41[illegible]5792)

I.D.L



Widener Library
3 2044 092 908 219